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HUME'S THEORY OF MENTAL ACTIVITY

EVER since Norman Kemp-Smith's brilliant paper on "The Naturalism of Hume" fifty-five years ago,[1] students of philosophy have acknowledged the fact that David Hume's *Treatise of Human Nature* is very much more than an encyclopedia of skepticism. Hume, it is now realized, was engaged in Book I of the *Treatise* in a serious attempt to answer some of the central problems of philosophy. However, there is rather less clarity about just what his answers were. No doubt the obscurity of Hume's positive theory is due in part to his smooth and jesting style, free of the ponderous terminology which in other works alerts the reader to the least touch of a theory. But I think the real reason for Hume's failure to get across his very novel suggestions is the fact that they carry him beyond the limits of his own system, so that he is forced to express his best ideas in language totally unsuited to them. To put the point in a sentence, Hume began the *Treatise* with the assumption that empirical knowledge could be explained by reference to the contents of the mind alone, and then made the profound discovery that it was the activity of the mind, rather than the nature of its contents, which accounted for all the puzzling features of empirical knowledge. This insight, which was so brilliantly exploited by Kant, and has become today a focus of attention through the studies of disposition terms and language habits, was used by Hume to clarify the nature of causal inference and to explain the origin of our concepts of material objects.

In this paper I shall try to extricate Hume's theory of mental activity from the associationism and copy theory of ideas in which it is embedded. Hume nowhere sets out whole the theory which I attribute to him, but every part of my interpretation, with the exception of several terminological clarifications, is amply supported by passages in the *Treatise*. I have tried to exhibit the *Treatise* as more than a dated work of purely historical interest.

[1] N. Kemp-Smith, "The Naturalism of Hume," *Mind*, XIV (1905), pp. 149-173; 335-347.

If my interpretation is correct, Hume can still make a useful contribution to current epistemological and metaphysical debates.

I

The best way to get hold of Hume's new theory is to discover the precise point at which the framework of his system distorts his exposition and forces him to cramp his thoughts into ill-suited categories. That point, in my opinion, is reached when Hume attempts to explain the necessity of causal inference by appeal to "impressions of reflection." It is worth our while to explore a bit Hume's discussion of this category of impressions, for through it we will see what he was trying to say and why he had such difficulty saying it.

There are, Hume tells us, two sorts of impressions. The first are impressions of sensation, which "without any introduction make their appearance in the soul" (p. 275).[2] These are dependent upon "natural and physical causes," the examination of which, Hume says, would lead him "into the sciences of anatomy and natural philosophy" (p. 276). He therefore turns to the second category: impressions of reflection. It sometimes happens that an idea, which in its turn is derived from some precedent impression, will "return upon the soul" (p. 8) to produce new impressions, of pride, humility, ambition, vanity, hope, fear, desire, aversion, or any of the countless "passions and other emotions resembling them" (p. 275; also pp. 276-277). The category of impressions of reflection finds its most natural employment in Books II and III of the *Treatise*, where Hume discusses the passions and moral sentiments. Kemp-Smith has based on this fact his claim that Books II and III were earlier in composition than Book I.[3] According to Kemp-Smith, who seems to me quite convincing, the analyses of causation, space and time, and material objects are intended by Hume as extensions of a theory first advanced in connection with problems of ethics. The beliefs

[2] All parenthetical references are to *A Treatise of Human Nature*, ed. with analytical index by L. A. Selby-Bigge (Oxford, 1888).

[3] Cf. N. Kemp-Smith, *The Philosophy of David Hume*, 2nd ed. rev. (London, 1923), pp. 12-20, 44-46.

arising from the "impression" of causal necessity are viewed as analogous to the moral sentiments produced in the soul by the mechanism of sympathy. "*Belief*," says Hume, "*is more properly an act of the sensitive, than of the cogitative part of our natures*" (p. 183, italics Hume's).

There are two important similarities between moral sentiment and passions on the one hand and empirical beliefs on the other, which seem to me to have led Hume to group them together as "impressions of reflection." First of all, Hume believed that neither passions nor beliefs are "rational" in the strict sense of "justifiably derivable from relations of ideas" (cf. p. 70). Passions, of course, are not the products of reasoning, nor, thought Hume, are the sentiments of approbation and disapprobation. Now the whole point of his attack on the rationalists was to show that empirical beliefs are also not justifiable by an appeal to relations of ideas. They spring from "nonrational" sources in the soul. It is for this reason that Hume wrote, "Belief is more properly an act of the sensitive, than of the cogitative part of our natures." Second, and in my opinion even more important, Hume believed that moral sentiments, passions, and empirical beliefs are all responses of the mind to the presented world rather than given contents of experience. Just as love, hatred, approval, and disapproval are second-level reactions of the mind to experience, so also the beliefs in causal necessity and physical objects result from the mind's "reflection" upon its sensations. What I suggest is that these similarities of beliefs to passions and moral sentiments led Hume to identify them and to use in Book I the tool—impressions of reflection—which had worked so well in Books II and III. For notice that impressions of reflection are (1) nonrational in origin, and (2) contributed by the mind to experience rather than derived directly from perception. These are just the ways in which passions, moral sentiments, and beliefs are similar, on Hume's view.

But if there are similarities, still there are striking differences. Perhaps the most troubling difference is that the so-called "impressions of reflection" cited by Hume to explain the origins of empirical beliefs are remarkably elusive and hard to find. Hume feels no need to prove the existence of love or anger or

desire. But when it comes to the "impression of necessary connexion" he does not simply say, "Look and see." Rather, he eliminates alternatives, argues by *reductio ad absurdum*, and generally does just what we would expect of a philosopher attempting to pin down an unknown quantity. The reason for this odd behavior is not difficult to discover. Hume began the *Treatise*, as I have suggested, by assuming that all empirical concepts could be explained in terms of some combination of the contents of perception. He very quickly came to see that knowledge and belief result from what the mind *does* with its contents rather than simply from the nature of those contents. Hence, most of Book I is devoted to a discussion of the activities of the mind. Having no category in which to put mental activities, however, Hume tried to squeeze them into the slot labelled "impressions of reflection." Now in order to remove some of the confusion thus caused, we must attempt a systematic restatement and interpretation of Hume's discussion of mental activity. After a general statement, we can look to the text for confirming evidence.

II

Hume begins his analysis of the mind's operations with an appeal to associations of ideas. Modeling his discussion on the Newtonian theory of gravitation, he attempts to explain the phenomena of cognition without referring to secret causes and without framing hypotheses concerning ultimate qualities (p. xxi). The central thesis is that there is a "uniting principle" among ideas which can be regarded as a "gentle force," influencing the imagination in its arrangements and rearrangements of perceptions (pp. 10-11). It is at this point unclear whether some special condition of the mind is necessary for the working of the force. Hume's emphasis is all on the nature of the ideas themselves. The imagination, if subjected repeatedly to the gentle force of association, develops certain *habits* or *customs*. It comes to anticipate the conjunction of perceptions which past experience has exhibited. Much as Pavlov's dog would begin to salivate at the sound of a bell, so the mind generates the idea of an "effect" if presented with the "cause." Indeed, says Hume, the habit of

thinking an idea when presented with an impression is all there is to causal inference. The gentle force, so unassuming in its first appearance, is the adhesive for all experience.

This explanation, based on an analogy between gravitation and association, is not satisfactory as it stands. According to Newton, two bodies attract one another without (so far as we know) the intervention of any third thing. This is at least intelligible, for bodies can literally move about, toward or away from each other. But an impression clearly is not a body which approaches or recedes from other impressions. When Hume says that the cause and effect are "associated," he means that the mind tends to think of one when presented with the other. Thus the metaphor of a "gentle force" is misleading. The impressions affect the mind, not one another. The question remains, by what means does the observed contiguity and resemblance become translated into a habit of association? Then, too, the theory of association fails to explain the striking similarity in the habits of different minds, or of one mind at different times. Despite a bewildering variety in perceptual experience, the mind, according to Hume, seems always to come up with the same few types of association: causes, objects, the self. These are only a few of the inadequacies of the simple associationism of the early pages of the *Treatise*. Hume soon moves past this theory to a more complicated analysis of cognition, basing his account on a theory of mental propensities.[4]

A "propensity" can be described as the disposition to develop a disposition, or as a "second-level" disposition. When we attempt to explain the behavior of Pavlov's dog, for example, it is not enough to state that Pavlov rang the bell every time the food was offered. That is, of course, a necessary part of the explanation, but we must add that the dog was *capable* of being conditioned. If it were not, no amount of bell-ringing could produce the conditioned reflex of salivation. Let us use the term "disposition" to describe the fact that an entity is prone to act or react in certain ways under certain conditions. Let us use the term "propensity" to describe the fact that an entity is prone to *develop certain disposi-*

[4] What follows is a reconstruction of Hume's view. In the third and fourth sections of this paper I have assembled the evidence for its defense.

tions under certain conditions.[5] Then we can say that the dog's salivation upon the ringing of the bell is a manifestation of a disposition (in this case a conditioned reflex) and the disposition in turn is a manifestation of the dog's propensity (to form dispositions of this sort).

There are several simple facts about stimuli, dispositions, and propensities which it will prove helpful to keep in mind when considering Hume's theory. First, the stimuli are, of course, sensory in nature. They are the instigators or "proximate causes" of the conditioning process, as well as the triggers of the disposition already formed. Second, the sensory stimuli are the individuating conditions of the dispositions being formed. The nature of the stimulus determines the precise character, or content, of the disposition. For example, by choosing a different stimulus, we can make the dog salivate at the blowing of a whistle or a clap of the hands rather than at the ringing of a bell. Third, dispositions are distinguished from propensities by their logical type. As mentioned above, propensities are second-level dispositions, or dispositions to form dispositions. Therefore, dispositions depend upon the conditioning stimuli and follow after them, while propensities precede both stimuli and dispositions. Propensities are thus necessary conditions for the development of dispositions.

With these few points outlined, I can now state the theory of mental activity which I claim to find in Book I of the *Treatise*. The proof of my claim, of course, will come in the detailed examination of Hume's account of our concepts of causal inference and material objects. Put briefly, the theory runs like this: The human mind has a small number of innate propensities, or "dispositions to form dispositions." When the mind is presented with perceptions conjoined in certain ways, its propensities are activated and it develops dispositions. These dispositions determine the mind to reproduce in imagination certain impressions when it experiences certain others.[6] The mind, in another of

[5] This is not the usage followed by Hume. He does not observe strict distinctions among "habit," "custom," "propensity," and "disposition."

[6] The impressions, of course, are reproduced in a less vivid form, as ideas (p. 93).

Hume's phrases, forms a "habit of association." The factors in cognition which Hume labeled impressions of reflection—such as the impression of necessary connection—are really dispositions, and the ideas of necessary connection, substance, and so forth, are not copies of impressions but ideas of mental dispositions. The innate propensities constitute the basic "machinery" of the mind. They are the necessary and universal conditions of all our ideas of causes and objects. In Hume's words:

> In order to justify myself, I must distinguish in the imagination betwixt the principles which are permanent, irresistible, and universal; such as the customary transition from causes to effects, and from effects to causes: And the principles, which are changeable, weak, and irregular; such as those I have just now taken notice of. The former are the foundation of all our thoughts and actions, so that upon their removal human nature must immediately perish and go to ruin [p. 225].

Now let us examine the arguments of the *Treatise* in more detail.

III

I shall begin with Hume's discussion of causal inference, which occupies Part III of Book I. After completing his famous and devastating attacks on the rationalists, Hume translates the problem of causation into two questions: (1) what is the nature of the idea of necessary connection? and (2) "Why [do] we conclude that such particular causes must *necessarily* have such particular effects; and what is the nature of that *inference* we draw from the one to the other, and of the belief we repose in it?" (p. 78).[7] Hume chooses to answer the second question first, leaving the "idea of necessary connexion" for later.

When we examine specific causal reasoning, we find four components: an impression, an inference, an idea, and a belief. All causal reasoning begins with an *impression* of sensation which is

[7] Hume also asks "For what reason we pronounce it *necessary* that every thing whose existence has a beginning shou'd also have a cause?" (p. 78). However, he nowhere accounts for our belief in this "causal maxim," and this may be why he omits all mention of it from the *Enquiry Concerning Human Understanding*. Cf. Kemp-Smith, *The Philosophy of David Hume*, pp. 405-413.

present before the mind and acts as the anchor of the entire process. The second component is the *inference* by which the mind passes from the present impression to an *idea*. This idea, related to the impression, is the third component of the reasoning process. Fourth, there is the *belief* which we then repose in the idea.

The impression needs no explanation. To account for the inference to the idea, Hume introduces the factor of "constant conjunction." Upon examining causally related objects, and reflecting back over past experience, we discover that whenever we label one object cause of another, we can recall pairs of contiguous and successive objects which resemble the present impression and idea. As the objects appear again and again in similar relations to one another, they become "associated" in the imagination. By a natural mental process, the habit is inculcated of conceiving of the one when the other is perceived. When the impression of the cause, let us say, is present to the mind, then the disposition created by past conjunction determines the mind to reproduce the idea of the effect. The essence of the causal inference lies in this transition. There remains only the belief, which I discuss below.

Hume returns in Section 14 of Part III to the first question, What is the nature of the idea of necessary connection? His analysis of particular causal inferences has produced three conclusions which, he believes, permit him now to answer the question. The conclusions are

> that the simple view of any two objects or actions, however related, can never give us any idea of power, or of a connexion betwixt them: *that* this idea arises from the repetition of their union: *that* the repetition neither discovers nor causes any thing in the objects, but has an influence only on the mind, by that customary transition it produces [p. 166].

But every idea, says Hume, is a copy of some impression. As the idea of necessary connection is not derived from any quality or relation of objects, it cannot be a copy of an impression of sensation. The only available impression of reflection is the "feeling" attached to the disposition of the mind to pass from an object to

the idea associated with it (p. 165). Therefore the idea of necessary connection must be the idea of this mental transition. This analysis of causal inference depends on three points, each of which requires some comment. The points are: (1) the nature of the "transition" from impression to related idea; (2) the nature of the "impression of necessary connexion" on which we base our idea of causal influence; and (3) the nature of the mechanism of belief, whereby we assent to the inference and take the associated idea as objectively representative. Let us consider them in turn.

The "transition" of which Hume speaks is not to be understood as a passing of the mind from one present perception to another *present* perception. What happens is that the mind, perceiving the first, "forms an idea of its usual attendant" (p. 93). In other words, the mind reproduces in imagination a copy of the impression which has lately been associated with the present impression. This process of recall is the manifestation of a mental disposition or "habit" which is inculcated by experience. The mind is confronted with a succession of resembling impressions, and thereupon develops a disposition to conceive the one when it perceives the other. As I have already noted, the proneness of the mind to develop such a disposition is itself a disposition; it is what I called a "propensity." Hume now sets to one side the aspects of causal reasoning which are independent of the mind's inferences. The contiguity and succession of objects is indeed separate from our thought, as is the constant association of like objects (p. 168). These relations are "independent of, and antecedent to the operations of the understanding" (p. 168). But the necessity of their connection is a property of our inference upon them and derives from the mind's "habit." In the terminology which I have adopted, the associated impressions act as stimuli to activate an innate propensity; the result is a mental disposition to imagine a related idea when presented with an impression.

The second element of Hume's analysis is the "impression of necessary connexion" from which the idea of causal influence is copied. We are here presented with a common problem of textual interpretation. What Hume says is not the same as what Hume says he says. As I have suggested, the natural development of his argument leads him in one direction, but the prior strictures

of his system require a different and conflicting move. Hume talks as if the impression of necessary connection, like an impression of love or envy, were a directly observable mental content arising from the workings of the imagination. But the description which he actually gives of this impression is peculiar, to say the least. The "internal impression," he claims, is the "propensity, which custom produces, to pass from an object to the idea of its usual attendant" (p. 165). Again, the "customary transition is the same with the power and necessity" (p. 166). Hume is not saying here that the impression arises from the transition or is conjoined with the transition or is dependent upon the transition; he is saying that the impression *is* the transition. Now this is plainly an error in classification. "Customary transitions" and "propensities" are mental operations or powers, not contents of consciousness. If the idea of necessary connection is a copy of the transition from an impression to its usual attendant, then it is a copy of a mental activity. It is in fact the idea of the mind's disposition to reproduce related perceptions in imagination. This is just the muddle which I referred to above, when discussing impressions of reflection. In these passages we can observe Hume shifting toward explanation in terms of mental activities, while still tied to the language of mental contents. Later on we shall see him move even further in this direction when he deals with the problem of external objects.

Finally, let us consider the problem of *belief*. Hume holds that to believe in the existence of an object is not to conjoin an idea of existence to the idea of the object (p. 94). He casts about, therefore, for some quality common to all the ideas and impressions in which we repose belief, and he hits upon "the *manner* of our conceiving them." The belief in an idea is nothing but an increased force and vivacity by which our conception is heightened and enlivened (p. 96). This theory, as Hume himself recognized, is open to serious objections. One of these is the fact that there are many common beliefs which cannot be explained as "enlivened ideas." When we form an inference upon the evidence of historical traces, and reason from a present impression of manuscripts to the past existence of a historical personage, the resultant belief cannot be attributed to the force of the impressions

of the text before us (pp. 145-146). We could not, for example, strengthen the belief by illuminating the page more brightly or recasting the print more sharply.[8] Hume appears to take account of this objection in several passages, the first of which contains a turn of phrase suggesting a possible revision:

> For suppose I form at present an idea, of which I have forgot the correspondent impression, I am able to conclude from this idea, that such an impression did once exist; and as this conclusion is attended with belief, it may be ask'd, from whence are the qualities of force and vivacity deriv'd, which constitute this belief? And to this I answer very readily, *from the present idea.* For as this idea is not here considered as the representation of any absent object, but as a real perception in the mind, of which we are intimately conscious, it must be able to bestow on whatever is related to it the same quality, call it *firmness, or solidity, or force, or vivacity*, with which the mind reflects upon it, and is assur'd of its present existence [pp. 105-106].

The key phrase is the last clause but one of the final sentence. The present idea, serving as the "impression" from which the inference proceeds, must possess that firmness or solidity or force or vivacity *with which the mind reflects upon it.* The force is not a quality of the perception, but rather a quality of the way in which the mind conceives it. Hence any idea or impression, however weak, may serve as the starting point for a causal inference, if only the mind reflects upon it with the solidity of belief. Now in the passage I have quoted, Hume uses this locution only to explain our inference from a present idea to the past impression *of which it is a copy*. But in the Appendix he extends the same description to all belief. There he states that the "feeling" of belief is a "*firmer conception, or a faster hold, that we take of the object*" (p. 627; cf. p. 626). Belief is not a hold that the object takes on us, but that we take on the object. Here again we see a shift in emphasis from the characteristics of perception to some activity of the mind.

[8] It is the existence of the beliefs, not their truth, which is at stake here. The trouble with Hume's theory is that it fails to explain why we do *not* believe vivid and affecting fiction, and yet *do* believe dull history books.

IV

Hume's theory of mental activity reaches its fullest development in his treatment of our belief in material objects. The argument is carried on in several different passages in Book I of the *Treatise*, and a great deal of restatement and interpretation is needed to present it in coherent form. The first passage is a discussion of substance which comes as part of an examination of relations, modes, substances, and abstract ideas in Part I. It is little more than a page in length, and the laconic manner in which it proceeds suggests that Hume did not consider his views especially original. Nevertheless, his remarks are exceptionally suggestive, albeit brief.

What is our idea of substance? asks Hume (p. 15).[9] Does it derive from an impression of sensation or of reflection (p. 16)? Surely neither, he answers, for no one impression is the origin of the idea of a substance. Hence it must be an idea of "a collection of particular qualities" (p. 16). To the qualities, or their ideas in our mind, is attached a name, by which we may recall all or some of them when necessary. As we learn more about those qualities which bear an intimate relation to the given collection, our idea of it expands, and the number of ideas which its name recalls becomes larger. Thus, a first idea of gold may include the qualities yellow, hard, and heavy. When the quality of solubility in *aqua regia* is learned, this is added to the collection, and henceforth the word "gold" is capable of calling it to mind.

Now Hume adds a highly significant sentence, which unfortunately is left without comment: "The principle of union [of the qualities] being regarded as the chief part of the complex idea, gives entrance to whatever quality afterwards occurs, and is equally comprehended by it, as are the others, which first presented themselves" (p. 16). Ideas of substance are distinguished by this fact from ideas of modes. Modes are also collections of qualities, to be sure. But the qualities are either not closely connected by the relations of contiguity and causation—Hume offers the example of a dance—or else are united together, but in such

[9] He means, what is our idea of *a* substance; that is to say, what is our idea of gold or wood or a man?

a way that "the uniting principle is not regarded as the foundation of the complex idea" (p. 17). To introduce a *new* quality, therefore, destroys the particular unity to which the name is attached, and occasions a new name. Thus an arrangement of colors may be beautiful, but the beauty is not a substance. For if it were, one could attach other related qualities to the collection without destroying the idea of the beauty. What in fact happens, of course, is that any addition or subtraction produces a *new* idea—of an equal beauty, perhaps, but *different* from the old idea (p. 17). On the other hand, one can subtract from or add to the qualities of a substance without thereby destroying its unity. A man is still the same substance whether he gains weight, cuts his hair, gets a tan, or even loses several limbs. So, too, for other substances. They remain the "same thing" through a variety of changes. Indeed, on Hume's view it would make no sense to speak of learning about a *thing*, unless our concept of it were in some way more than an idea of the qualities we conceive it to possess. For otherwise an added quality would be part of a *new* thing, not a new quality of the old thing.

Three questions are raised by Hume's preliminary analysis of the idea of substance. First, what is the "principle of union" by which the imagination unites the qualities of a substance? Second, how does the imagination form the principle of union? And third, what does Hume mean by his statement that the principle of union is the "chief part" of the idea of a substance?

The principle of union is described by Hume as being the manner in which the various qualities "are united by the imagination, and have a particular name assigned to them, by which we are able to recall, either to ourselves or others, that collection" (p. 16). This description is so remarkably similar to the account of "abstract ideas" given in the very next section of the *Treatise* that I shall draw on that account for an expansion of Hume's brief analysis of substance. Why he did not himself unite the two is a mystery, for he would thereby have strengthened considerably his entire theory of our concepts of objects.

In the light of the section "Of Abstract Ideas" we can construct the following interpretation of Hume's view of the unity of a

substance. The "principle of union" is a habit or custom of the mind to which is attached a word. The habit determines the mind to reproduce in imagination one or more of a set of ideas, when the word for that set has been uttered or thought or otherwise invoked. Once the mind has formed the habit, it can reinvoke it at will, and by "one of the most extraordinary circumstances" of the mind's powers (p. 21), a false assertion about the membership of the set will often provoke the mind to reproduce just that member which will effectively belie the ascription. Thus, if the mind ascribes the quality of dryness to water, it will immediately recall the idea of wetness without running through the qualities of coldness, lucidity, and so forth. It will, furthermore, recognize the idea of wetness as having been called forth by the habit associated with the word "water." Hume does not pretend to know how or why this delicate capacity is possessed by the mind. He says that "To explain the ultimate causes of our mental actions is impossible. 'Tis sufficient, if we can give any satisfactory account of them from experience and analogy" (p. 22).

Returning to Hume's example, we see that the mind has formed the habit of calling up yellowness, weight, malleability, and fusibility when it conceives the word "gold." These several ideas are united solely by the mind's habit, though they are *associated* by the relations of contiguity and causation (p. 16). We are brought then to the second of our three questions: how does the imagination *form* the habit which unites the qualities? The full answer to this question is only developed by Hume in Part IV of Book I, which I shall examine presently. In this earlier section, however, he indicates the line which his argument will take. The difference between the qualities of a substance and those of a mode, he tells us,

> consist[s] in this, that the particular qualities, which form a substance, are commonly refer'd to an unknown *something*, in which they are supposed to inhere; or granting this fiction should not take place, are at least supposed to be *closely and inseparably connected by the relations of contiguity and causation* [p. 16; italics mine].

As in the case of causal inference, the mind forms a habit of

reproducing perceptions as the result of certain perceived relations of objects.[10]

Finally, in what sense is the habit or principle of union the chief part of the idea of a substance? A possible answer is that Hume means to call the principle of union a *necessary condition* of the idea of the substance. The idea, to be sure, must contain some quality-ideas, for otherwise it would consist of a principle of union with nothing to unify. The mind would have a habit of reproduction which did not dispose it to reproduce anything at all. But among the several qualities which we impute to a substance, no single one is essential to our conception. One man's idea of gold may include the qualities yellowness, heaviness, malleability; another's conception might omit the color yellow and yet include solubility in acid, and so on. The principle of union, on the other hand, *is* a necessary element in the idea of any substance. It is for this reason that it is the "chief part" of the idea. If it is omitted, the mind is left with an unstructured assortment of ideas which are not bound up in any manner warranting the assignment of a special name.

The major discussion of the belief in material objects occurs in the long section, "Of scepticism with regard to the senses," which forms the core of Part IV. Hume defines his subject as "the *causes* which induce us to believe in the existence of body . . .," for, as he says a few lines earlier:

> We may well ask *What causes induce us to believe in the existence of body?* but 'tis in vain to ask *Whether there be body or not?* That is a point, which we must take for granted in all our reasonings [p. 187].

Specifically, it is our belief in the *continued* and *independent* existence of bodies which needs explanation. As this belief does not arise from the impressions alone, it must arise "from a concurrence of some of their qualities with the qualities of the imagination" (p. 194). Hume discovers two kinds of regularity in our impressions: constancy and coherence. Constancy is the simpler, for it

[10] The analysis is complicated by the fact that causal belief, itself the result of a habit of the imagination, plays here the role of stimulus. Equally, causal belief involves belief in the existence of objects which are causally related. Hume never succeeded in sorting out the complicated relationships between causal and substantial beliefs.

depends solely upon the repetition of resembling impressions, while coherence depends upon a second order regularity, namely, constancy of the principle of alteration. Nevertheless, Hume discusses coherence first.

It frequently happens that an impression which has in the past been regularly associated with another will make an appearance in perception alone, thereby "contradicting" past experience. For example, the sound of a squeaking door, if not accompanied by a sight of the door, runs against the past experience in which the two have been conjoined. In order to preserve the coherence of my experience, I assume a door (or, better, the visual aspect of a door) to exist, even though I do not perceive it. This would seem to involve a new propensity of the mind (p. 198).

But, Hume announces, the propensity to preserve and extend coherence is very different from the customs and habits which explained causal belief (p. 197). Mere habit can explain a belief only in what is actually inculcated by experience. If we condition a parrot to say "Two plus two is four," knowing that it has the capacity to learn simple phrases, we ought not to be surprised if it speaks the words back. But if, one day, the bird is heard to say "Two plus three is five," then we shall either discover who has been secretly coaching it or be very surprised indeed. Now our believing in the continued existence of objects is like the parrot saying "Two plus three is five," for although experience teaches us to expect coherence in the impressions we perceive, it does not, and obviously could never, teach us to expect perceptions to continue to exist when we do *not* perceive them. The mind here actually imparts to its perceptions a *greater* regularity than they naturally possess.

This is the first suggestion of the manner in which the causal connections of perceptions lead to our conceptions of objects. Taking Hume's example, the mind is accustomed to perceiving the movement of the door together with the squeak, *which is its effect*. When presented only with the squeak, the mind makes a causal inference in the manner described above, concluding that the door exists though it is unperceived. This in turn supports the causal belief, for the isolated squeak can then be interpreted

as a positive, rather than a negative, instance of the causal relation. Were the mind limited merely to its causal beliefs, the force of the connection would be weakened. By creating the "fiction" of a continued and independently existing object, the mind is enabled to preserve and increase the order of its perceptions. In a manner of speaking, the mind's propensity reinforces the causal inference, protecting it from the excessive disconnectedness of experience. The two propensities together subdue a chaos which would overwhelm the "causal" propensity and greatly diminish its effectiveness.

The second characteristic common to "objective" impressions is the constancy with which they reappear in experience. We must explain the effect of this constancy upon the mind, says Hume, for coherence and the propensity it invokes are "too weak to support alone so vast an edifice, as is that of the continu'd existence of all external bodies" (p. 198-199). Hume summarizes his explanation before presenting it in detail:

> When we have been accustom'd to observe a constancy in certain impressions, . . . we are not apt to regard these interrupted impressions as different (which they really are) but on the contrary consider them as individually the same, upon account of their resemblance. But as this interruption of their existence is contrary to their perfect identity . . . we . . . are involv'd in a kind of contradiction. In order to free ourselves from [it we suppose] that these interrupted perceptions are connected by a real existence, of which we are insensible [p. 199].

As Hume intends to explain why we impute a continued and independent existence to objects, he must first make clear what we mean when we speak of various impressions as being the impressions of *an object*. Although the perceptions are distinguishable (and hence discrete) we treat them as one, supposing the object to be identical with itself at different times. But one impression gives the idea of *unity*; several impressions give the idea of *number* or multiplicity.

> Betwixt unity and number there can be no medium. . . . After one object is suppos'd to exist, we must either suppose another also to exist; in which case we have the idea of number: Or we must suppose it not to exist; in which case the first remains at unity [p. 200].

The escape from the dilemma lies in the relation of time to our awareness of mental contents. Time is the idea of the manner in which successive impressions appear to the mind. Now imagine a series of exactly similar impressions, among which the mind distinguishes no mark of difference. If attention is paid to the passage of time (by noticing the alteration of other impressions, for example) then we distinguish among the members of the series and conceive the idea of number.[11] If only their invariable and uninterrupted similarity is attended to, then we conceive the idea of unity. The idea of identity is a mixture, or confusion, of the ideas of number and unity. As Hume puts it:

> By this means we make a difference, betwixt . . . object and . . . itself, without going to the length of number, and . . . without restraining ourselves to a strict and absolute unity [p. 201].

But experience all too rarely provides the mind with conditions for the conception of identity. Even resembling impressions are interrupted in their appearance. Hume now explains why the mind extends its identity-judgments to these faulty series.

The resemblance of impressions induces in the mind a habit of recollecting them together. The habit or disposition by which the mind recalls an uninterrupted series is, to be sure, different from the habit by which it recalls a discontinuous series. But the two dispositions are *alike* (p. 203), and just as the mind is prone to associate like impressions, so it is prone to confuse similar dispositions. "Whatever ideas place the mind in the same disposition or in similar ones," Hume says, "are very apt to be confounded" (p. 203).

No sooner has the mind confused its several dispositions and denominated the discontinuous impressions "identical" than it is thrown into the baldest contradiction. The successive impressions are obviously *not* continuous and uninterrupted. Hence they ought not to be called identical (p. 205). Confronted with this conflict, the mind chooses the bolder alternative, and rather than

[11] Though Hume does not say so, the distinction is presumably a "distinction of reason" (pp. 24-25). Successive resembling impressions are unlike in being dissimilar to a third impression, say one which is *contemporaneous* with the first, but *precedent* to the second.

declare the impressions different, "unite[s] these broken appearances by the fiction of a continu'd existence" (p. 205). Apparently this means that the mind, in recollecting the discontinuous series, "fills in" the lacunae by reproducing other resembling perceptions in positions where none were experienced. Hume says:

> Our memory presents us with a vast number of instances of perceptions perfectly resembling each other, that return at different distances of time, and after considerable interruptions. This resemblance gives us a propension to consider these interrupted perceptions as the same; and also a propension to connect them by a continu'd existence, in order to justify this identity, and avoid the contradiction, in which the interrupted appearance of these perceptions seems necessarily to involve us [pp. 208-209].

In the terminology which I have employed, the mind approaches experience with the propensities which, in the case of each object-belief, operate to develop two dispositions. The dispositions together produce the idea of a continued and unified object.

V

Now let us pull together the results of the analysis of Book I, and see whether the interpretation outlined in Section II of this paper has been substantiated. It was there suggested that Hume had developed a theory of mental activity in which the key elements are certain innate propensities, and the dispositions which result when those propensities are "activated" by sensation. I think it is now clear that the various "principles" invoked by Hume do have the characteristics of dispositions and propensities. Consider first the role of sensation in the formation of empirical belief. The mind is presented with a variety of impressions which rapidly come and go in regular patterns. Stimulated by these perceptual regularities, certain mental propensities are activated, and the mind becomes disposed to reproduce its perceptions in imagination according to some established rule. Thereafter, this disposition can be "touched off" by the appearance of a suitable impression, which acts as stimulus to the mind. The similarity to the conditioning of Pavlov's dog is evident: first the bell and salivation together (first the cause and

effect together), then the bell alone (the cause alone), and by virtue of the conditioned reflex (the mental disposition) the salivation (idea of the effect) occurs (is produced).

Furthermore, the impressions serve as the individuating factors of a disposition. For example, the disposition to recollect together the properties of gold may be based on a general propensity to develop such "substance-dispositions," but that the mind should associate hardness and malleability with *yellow* rather than with *green* is a result of the particularities of sensory experience. Were the patterns of perception different, the mind would associate together different qualities. It is easy to imagine a well-ordered, comprehensible world in which fire is cold, rocks are soft, gold is brittle, and water tastes like honey. But in such a world, there would be causes and effects and there *would* be continuous independent objects,[12] for our conception of causes and objects depends on the propensities of the mind itself. Without them, Hume tells us, "human nature [would] immediately perish and go to ruin" (p. 225). Thus the impressions of sensation are the stimuli which activate propensities and individuate dispositions. The "permanent, irresistible, and universal" principles, or propensities, lie ready for experience. The dispositions, on the other hand, wait upon experience, for their individual nature is determined by the qualities of the impressions.

Finally, these propensities and dispositions are all *mental* principles. Hume at times conceals this important fact by his associationistic language. In some passages he seems to suggest that perceptions are attracted to one another by a "gentle force" of association (pp. 10-11), without the interference of the mind. When he actually comes to describe the "transitions" and "principles of union," however, he makes it clear that the transition is a transition *of the mind* from one perception to another; that the principle of union is a principle by which the imagination recalls a set of perceptions; and in general that the propensities which precede experience and the dispositions which result are mental pronenesses to reproduce perceptions in imagination.

We are now in a position to make a list of the propensities which

[12] More properly, there would be causal *beliefs*, and object *beliefs*.

Hume describes in Book I of the *Treatise*. Hume never groups them together in this fashion, but if he had, the result might well have been labelled a "Table of Categories," for the propensities actually play a role quite similar to that of the categories in the *Critique of Pure Reason*. The list is as follows.

1. The propensity to develop, under the stimulus of repeated conjunction of resembling pairs of objects, a disposition to reproduce the idea of the one when presented with the impression of the other.[13]

2. The propensity to develop, under the stimulus of a set of impressions which (by the first propensity) are conceived as causally interrelated, a disposition to reproduce the impressions together, and by confusing the related set with a series of resembling impressions, to conceive the reproduced perceptions as identical.

3. The propensity to develop, under the stimulus of a causal inference from a present object to an absent object, a disposition to conceive that absent object as existent, and hence to reinforce both the causal inference and the belief in continued existence of objects.

4. The propensity to develop, under the stimulus of a discontinuous series of resembling impressions, a disposition to reproduce the series as if it were continuous, filling in the lacunae with suitable ideas, and thus permitting the ascription of *identity* and *continuity* to the series.

5. The propensity to develop, under the stimulus of a present perception, a disposition to conceive an associated perception with a greater firmness of belief, the more firmly the present perception is conceived and the closer the association between them.

The first propensity is responsible for our causal inferences. The second, dealing with the "principle of union," the third, which depends on "coherence," and the fourth, whose stimulus is "constancy," are jointly responsible for our conception of

[13] The clumsiness of these definitions is occasioned by the attempt to include in them all three of the elements of mental activity: propensity, stimulus, and disposition.

unified, independent, continuous objects. The fifth propensity is the source of the *belief* in causal influence and an external world. Taken together, these five propensities comprise the principles which Hume calls "permanent, irresistible, and universal" (p. 225). The first four propensities determine *which* perceptions the mind reproduces, and the fifth determines the *manner* in which they are reproduced.

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POSITIVE "ETHICS" AND NORMATIVE "SCIENCE"

THE relation between ethics and science has been a focal point in the arguments of many moral philosophers. The noncognitivists' doctrines of the primarily emotive or prescriptive meaning of ethical terms have derived in part from the sharp contrasts they have drawn between ethics and science. Evidence for these contrasts has been found in such considerations as the difference between moral and scientific disagreements in respect of cognitive decidability. Many of the cognitivists, on the other hand, have emphasized the similarities between ethics and science. These emphases have taken two closely related forms. Some philosophers have tried to show that ethics is "scientific" in that ethical terms, sentences, and arguments, like scientific ones, are respectively descriptive, cognitive, and rational (appealing to reasons and principles). Other philosophers have tried to show that science is "ethical" in that science, like ethics, is attitudinal (exhibited in the scientific "attitude," "respect" for the facts, and so forth), prescriptive (giving directions as to how to look at the world), evaluative (weighing evidence), purposive, pragmatic, socially structured, resting on conventions, and even "personal" in that science involves basic choices or commitments on the part of the scientist.

I. The Fallacy of Disparateness

The purpose of this paper is not to participate directly in this debate, but rather to call attention to a fundamental feature of its logic. This feature consists in a disparity between the levels at which, or the respects in which, ethics and science are characterized by many participants on each side, although more particularly perhaps by the noncognitivists. I shall first briefly illustrate this disparity in a rather obvious form and shall then devote the bulk of this paper to a critical examination of a less obvious but much more far-reaching form of it.

In discussing the moral disagreement between Bentham and those who opposed his hedonism as a "pig's philosophy," Russell comments as follows:

> Neither he nor his opponents could advance any argument. In a scientific question, evidence can be adduced on both sides, and in the end one side is seen to have the better case—or, if this does not happen, the question is left undecided. But in a question as to whether this or that is the ultimate Good, there is no evidence either way; each disputant can only appeal to his own emotions, and employ such rhetorical devices as shall rouse similar emotions in others.[1]

It will be noticed that in this passage Russell contrasts a moral disagreement over "the ultimate Good" with a disagreement over "a scientific question" as if these were logically on a par. But obviously they are not. Questions of "ultimate" principles in any field, including science as well as ethics, are not on the same level as intermediate questions in regard to which the relevant principles are assumed; for in the former case one cannot appeal to higher principles in order to answer them. Consequently, in comparing scientific with moral disagreement, Russell should have balanced his moral example about "the ultimate Good" with a scientific example of disagreement over an ultimate scientific principle. This would have strongly affected his contrast between the cognitive decidability of scientific disagreements and the undecidability of moral ones. It might even have led him to

[1] *Religion and Science* (London, 1935), p. 229. In some of his later writings Russell has recognized that the issue of the relation between moral and scientific disagreement is more complex than the above statement suggests, but he has not shifted his basic position. Cf. *Human Society in Ethics and Politics* (London, 1954), pp. 80, 81, 100-118. For essentially similar contrasts between moral and scientific disagreement, see C. L. Stevenson, *Ethics and Language* (New Haven, 1944), pp. 2 ff.; A. J. Ayer, *Language, Truth, and Logic*, 2nd ed. (London, 1948), pp. 21-22; *Philosophical Essays* (London, 1954), pp. 236-237; P. Edwards, *The Logic of Moral Discourse* (Glencoe, Ill., 1955), pp. 182-192. The same contrast is found, in principle, also in such noncognitivists as Hare, Urmson, and Nowell-Smith when they emphasize the differences between the meaning of "good" and the varying criteria of its application, or between moral and empirical predicates. For here too this difference involves the contrast between the cognitive decidability of scientific or factual disagreements as to whether an empirical predicate is to be applied to some object, and the undecidability of moral disagreements by reference to cognitive considerations alone, as to whether an ethical predicate is to be applied to an object.

the suspicion that in science, too, disagreements over ultimate questions are not decidable by cognitive means alone.

While recognizing that this example from Russell might be held to involve many further issues, I intend for the present to use it only for the purpose of drawing a moral from it. The moral is this: in comparing ethics and science, or any other fields for that matter, one must be careful not to commit what I shall call the *fallacy of disparateness*—the fallacy of discussing one field on one level or in one respect and the other field on a quite different level or in a quite different respect. Or, to put it in affirmative terms, comparisons between fields or subject matters should always refer to the same level or respect in each case. If this precept is violated, then nothing conclusive follows about the relations in question nor, to that extent, about the fields being compared.[2]

II. The Fundamental Disparity

I now wish to call attention to a much more fundamental and far-reaching logical disparity in the approach which many moral philosophers, especially noncognitivists, take to the relation between ethics and science. This disparity involves their very use of the words "ethics" and "science," and hence the basic assumption of the contrasts they have tried to draw between ethics and science.

What I am referring to is this: when the noncognitivists, as well as other philosophers, characterize "science" and "scientific" questions and disagreements, they always give a "normative" interpretation of the words "science" and "scientific"; but when they characterize "ethics" and "ethical" questions and disagreements, they usually give a "positive" interpretation of the words

[2] Political rhetoric notoriously exhibits this fallacy. When politicians "point with pride" and "view with alarm," they frequently characterize their own policies on an ideal level while portraying those of their opponents on the level of its actual manifestations or its worst possibilities. Such rhetorical devices may even contain the truth, but if so, then the truth is presented on two different levels. For example, the Soviets exalt their own system by citing the Marxist philosophy or the Soviet Constitution with its libertarian provisions (ideal level), while they denigrate the "capitalist" system by repeating newspaper reports about racial discrimination and gangsterism in the United States (level of selected actualities). Although such devices are effective in rhetoric, I take it that philosophy differs from rhetoric.

"ethics" and "ethical." By "normative" interpretation of the words "science" and "scientific," I mean that the philosophers' use of these words always carries an evaluative connotation which serves to restrict the words' application, from among the various candidates for being regarded as "cognitive" or "scientific," to those which fulfill certain cognitive norms or value-criteria, such as logical consistency, empirical testability, explanatory fruitfulness. But their use of the words "ethics" and "ethical" carries no such evaluative connotation, and hence the application of these words is not restricted to those candidates for being regarded as "moral" or "ethical" which fulfill certain moral norms or value-criteria. For example, such philosophers do not regard both Christian Science and neurology as examples of "science," but they do regard the discourse of both Al Capone and Albert Schweitzer as examples of "ethical" language. Hence there is a logical disparity between the philosophers' uses of the words "science" and "ethics": they use "science" on the level of the fulfillment of certain restrictive norms, while they use "ethics" without restriction to the corresponding norms.[3]

This disparity emerges especially clearly from the analogy drawn by many moral philosophers between their "meta-ethics" (or "analytical ethics") and the philosophy of science. They regard these two philosophic disciplines as being logically on a par in that, just as the philosophy of science does not do the work of science itself but rather analyzes the meanings of scientific concepts and the methods of scientific inference and explanation, so meta-ethics does not do the work of ethics itself; that is, it makes no ethical judgments of its own but rather analyzes the

[3] These evaluative and restrictive features of the normative as against the positive uses of words are also exhibited in many other contexts. For example, in legal theory the word "law" is used positively when it is applied to any system of rules enforced in a community, regardless of whether the rules be just or unjust; but if the view is taken that the rules must be just ones before they can be called "laws," then the word is used normatively. Or, to take an example outside the moral sphere, if one says that Ingrid Bergman is not an actress, or not a "real" or "genuine" actress, because her character portrayals lack range and depth, then one is using the word "actress" normatively. If, on the other hand, one applies the word "actress" to any female performer on stage, screen, or other theatrical media, without restriction as to norms by which one evaluates her performance, then one is using the word positively.

meanings of ethical terms and the methods of supporting ethical judgments.[4] However, the noncognitivists carry out the part of this analogy pertaining to ethics in a way which is logically quite disparate from the interpretation they give to the part pertaining to science. The philosophy of science as they view it always involves an initial normative commitment and restriction; when it studies the nature and methods of science, it does not try to make its theories about science fit the common denominators of the meanings of words like "science" and "know" as used by Christian Science, phrenology, and astrology as well as by neurology, physiology, and astronomy. On the contrary, it restricts its purview to the latter kind of sciences, for it deals only with those disciplines whose propositions and procedures fulfill certain methodological requirements or norms. In excluding such pretenders to the name of "science" as Christian Science from their purview, then, the philosophers of science distinguish at the outset between good and bad science, or rather between genuine science and bogus or illegitimate science, which they would say is not science at all. But the noncognitivists' approach to the philosophy of ethics, or meta-ethics, is on a quite different level. Here they make no such normative restrictions or differentiations between good or genuine ethics and bogus or counterfeit ethics; on the contrary, they try to base their theories about ethics on the common denominators of the meaning of ethical terms as used by missionaries, cannibals, democrats, and Nazis. Thus, qua meta-ethicists, they insist on a strict ethical neutrality. Stevenson, for example, calls attention to the "detachment" with which he approaches ethics, and refers to his work as a "relatively neutral study";[5] Ayer similarly writes that "All moral theories . . .

[4] Cf. Stevenson, *op. cit.*, p. 1: "Such a study is related to normative (or 'evaluative') ethics in much the same way that conceptual analysis and scientific method are related to the sciences. One would not expect a book on scientific method to do the work of science itself; and one must not expect to find here any conclusions about what conduct is right and wrong." For a similar analogy of meta-ethics to philosophy of science, see J. Harrison, "Can Ethics Do Without Propositions?," *Mind*, LIX (1950), 361-362. Cf. also T. D. Weldon, "Political Principles," in *Philosophy, Politics and Society*, ed. by P. Laslett (Oxford, 1956), pp. 22 ff. I have examined the "meta-ethical" distinction in "Meta-Ethics and Normative Ethics," *Mind*, LXIX (1960).

[5] *Op. cit.*, p. 1.

in so far as they are philosophical theories, are neutral as regards actual conduct."[6]

In these references to neutrality the writers do not mean merely that, just as the philosopher of science as such does not himself do science or take a stand on competing scientific theories, so the philosopher of ethics does not himself do ethics or take a stand on competing ethical doctrines. For the detachment and neutrality of the meta-ethicist are far more extensive and generous than that of the philosopher of science. The latter does not himself pronounce on the scientific merits of the wave theory of light as against the corpuscular theory, but he does insist, when he discusses the nature of "science," on confining his purview to what is common to *such* theories as against the pronouncements of astrologists and other occultists. But the meta-ethicist is far more permissive in his view of the scope of that "ethics" whose meanings and methods he studies. He imposes no such initial restrictions on what he will regard as the "ethics" or "ethical language" relevant to his concerns. Thus R.M. Hare, for example, in discussing the meaning of "good," concerns himself indifferently with the ethical language of missionaries, cannibals, and communists;[7] J. O. Urmson with the rhetorical tricks of advertisers and moral reformers;[8] and Russell with the ethical views of Nazis as well as democrats.[9]

Now this generalizing approach reflects, of course, the correct conviction of such philosophers that there are certain aspects of meaning common to the ways in which words like "good" are used not only in "moral" contexts by cannibals, missionaries, communists, and so on, but even in "non-moral" contexts by carpenters referring to good augers and ballplayers referring to good throws. But these philosophers deal only with such generic features of ethical terms, and they hold that, so far as their own philosophic interest is concerned, there is no need to look for what differentiates the morally good, or normatively ethical, from other

[6] *Philosophical Essays*, p. 246.

[7] *The Language of Morals* (Oxford, 1952), pp. 148-149.

[8] "On Grading," in *Logic and Language*, II, ed. by A. Flew (Oxford, 1955), p. 182.

[9] *Human Society in Ethics and Politics*, pp. 80, 81.

meanings of these terms. If the philosophers of science were to operate in an analogous way, they would confine themselves to the aspects of meaning common to the ways in which such terms as "know" and "true" are used by Christian Scientists, neurologists, phrenologists, physiologists, astrologists, and astronomers; and they would refuse to show how the neurologist's "science" is different from that of the Christian Scientist, that is, what are the norms or criteria by which to distinguish genuine science from bogus science. But of course the philosophers of science do just the opposite; and this involves that they take a normative position as to what constitutes the genuinely "scientific." Thus the anomalous outcome is that while the philosophy of science is normative and prescriptive, the philosophy of ethics has become positivistic and descriptive.

III. Consequences of the Disparity

This disparity has some serious consequences. For one thing, it leaves the philosophical ethicist, as such, powerless to distinguish between good and bad ethics. If ethics is to be viewed purely positively—in the sense that wherever there is a dispute over what should be done, the philosopher is to describe and analyze the language of the dispute but is to make no discriminations as to where the normatively ethical lies therein—then he pursues as vacuous a course as if he were to describe with complete impartiality a debate between a Christian Scientist and a neurologist, calling the debate an example of "scientific controversy" and making no attempt to differentiate between good and bad science or scientific argument.

To be sure, the philosopher may still insist on pointing out the difference between logical and illogical arguments in this connection. But his criteria of the "logical" would be of the sort which the neurologist rather than the Christian Scientist would admit, so that here already the philosopher would not, strictly speaking, be "neutral" as between them. Hence the question arises: why does the philosopher refuse to be neutral as between the "logics" of the neurologist and of the Christian Scientist, while he insists on being neutral, qua philosopher, between the

"ethics" of the democrat and the Nazi? It seems to be because he regards the normative difference between the "logics" as a cognitively ascertainable one, while he holds that the normative difference between the "ethics" is not cognitively ascertainable. But it must be emphasized that this distinction between the cognitiveness of the principles of science, including logic, and the noncognitiveness of the principles of ethics is itself a consequence of the disparate approaches to science and ethics. If science and ethics were approached on the same level, both would turn out in basic ways to be equally cognitive or equally noncognitive. I shall now try to show this, first with respect to ethical principles and then with respect to ethical disagreements and decisions.

(1) *The cognitiveness of ethical principles.* The philosopher of science, as we have seen, defines and explicates science by reference to certain methodological requirements or norms. These norms may be of different kinds, but let us assume that ultimately they consist in the canons of inductive and deductive logic. Hence, these norms constitute the basic regulative principles of science; among other things, they provide definitions of what it is to be a "fact" and "evidence," "truth," "scientific explanation," and so forth. In other words, these norms define what it means to *know* and to have *scientific knowledge*. Hence, it is the at least possible conformity to these norms which defines what is to be meant when a sentence or a belief is called "cognitive." Consequently, it is circular to say that the basic principles of science are themselves cognitive; for it is these principles or norms which determine whether anything else is to be called cognitive. Moreover, these principles are a selection from among other possible principles—possible, that is, in the sense that they are espoused by people who claim to have "science" or "knowledge" by methods which are in important respects different from those grounded in inductive and deductive logic. These other methods include those of Christian Science, astrology, phrenology, tribal medicine-men, and many others. Each of these other methods has its own way of defining what is to be meant by "fact," "knowledge," and so forth. Hence, if any of these latter is to be called "noncognitive," it will be by reference not to *its* norms or principles but to those of some *other* way of viewing "science" and

"knowledge." To claim that any of these is "absolutely" non-cognitive is to ignore the relativity of all claims of cognitiveness to norms or principles which define what is to be meant by "cognitive."

This does not mean, however, that the claim of cognitiveness is arbitrary. Considerations will usually be adduced in support of one or another way of defining "knowledge." But these considerations are not *proofs* that one or another definition is "correct" to the exclusion of alternative definitions; they constitute only an explication of the grounds which lead to the upholding of one definition as against another. Hence, strictly speaking, the choice among different conceptions of "knowledge" or "science" cannot itself be said to be made by cognitive means.

Now the principles of ethics have a similar logical status. These principles consist in the ultimate norms on the basis of which one way of life or kind of character is regarded as morally good as against its alternatives, whether it be the ethics of Jesus, of Hitler, of the Dobuans, of Middletown, or others. Hence, it is these norms which define what is to be meant by "morally good," "ethical," and so forth. And just as it would be circular to say that one "knows" that certain norms or principles of science are the "true" ones or that they constitute "knowledge," so for a parallel but not identical reason it is circular to say that one "knows" that certain norms or principles of ethics are the "true" ones in the sense that they are the morally good ones. One cannot know this about the principles of ethics, for it is the conformity to these principles which constitutes the moral goodness of anything else.

In this way, then, the principles of science are no more "cognitive" than the principles of ethics. To hold the opposite is possible only if one adopts disparate approaches to science and to ethics, for example, if one approaches science by its intermediate theorems and ethics by its ultimate principles, or if one takes a normative approach to science as against a positive approach to ethics. In the latter case, one will *assume* the definition of "cognitive" provided by the norms of science which one accepts, and will ignore the existence of rival conceptions of science and hence of the cognitive; consequently, in the absence of compet-

itors, all of "science," including its principles, will be regarded as cognitive since the operation of the implicit "cognitive-making" definitions will be ignored. But since, on the other hand, the existence of rival conceptions of the "ethical" will be recognized, and since, as we have seen, the choice among these conceptions cannot ultimately be made by cognitive means, the principles of ethics will be regarded as noncognitive. To recognize the disparity of the noncognitivists' approaches to ethics and to science is thus to remove the grounds on which they have contrasted the principles of science and of ethics as respectively cognitive and noncognitive.

(2) *The cognitiveness of ethical disagreements and decisions.* Similarly it is this disparity which underlies the conclusions of many moral philosophers about the noncognitive character of ethical disagreements and decisions. These conclusions, far from being a consequence of logical or empirical analyses of ethical language, are in many respects rather a result of the positive, nonrestrictive approach which the philosophers in question take to ethics.

Consider again the parallel case of science. Suppose that the philosopher were to describe with complete impartiality a dispute between a Christian Scientist and a neurologist, calling the debate an example of "scientific controversy." He would then come up with the acute observation that these "scientists" are disagreeing in attitude as well as in belief; that since no *empirical* facts on whose factual character both agree remove their disagreement, their controversy must be rooted in a disagreement in attitude; and that consequently the characteristic feature and basis of "scientific" disagreement is disagreement in attitude.

The point, in other words, is that disagreements can be primarily cognitive and not emotive wherever, as in the sciences which the noncognitivist philosophers would recognize, there are already agreed-upon norms, and particularly norms as to the methods by which to solve problems and resolve controversies. To decide a cognitive issue in any field requires at least two things: first, knowledge of the relevant facts, and second, agreement on criteria or norms for decision. The reason why scientific questions can be decided solely by cognitive means is that, since we (and I here exclude the Christian Scientist, and so forth) agree on the norms

or criteria of scientific method, the chief thing that remains is to determine the facts by the use of this method, and such determination is hence a purely cognitive process. But this is also true in respect of ethical and political issues. If men agree on ethical or political norms or criteria, then they can come to agreement by considering the facts in the light of these norms, and such consideration, too, is a purely cognitive process. It is at least partly because the noncognitivists take for granted men's agreement on scientific norms but not on ethical ones that they can contrast science and ethics as respectively cognitive and noncognitive. But this assumption involves taking a normative approach to science and a positive approach to ethics. Among "ethical" men there is agreement on ethical norms, just as among "scientific" men there is agreement on scientific norms. Hence disputes among the former can be resolved by cognitive means just as can disputes among the latter.

To be sure, as Aristotle pointed out, the subject matter of ethics is a contingent one in that it involves not the nature of things independent of man but the effects of human habits, volitions, and institutions; moreover, ethics, being practical, is concerned with what is to be done in the particular case. From these points it follows that ethical theorems cannot be derived from ethical principles with the same degree of probability of application to their subject matter as is the case in physics.[10] This, however, is only a difference in the modality of ethical and natural-scientific propositions and arguments; it does not make the former any less "cognitive" than the latter.

IV. Can the Disparity be Justified?

It may, however, be contended that even though the noncognitivists' approaches to ethics and to science do involve the logical disparity to which I have called attention, and that it is this disparity which underlies their conclusions about ethical language, still in none of this do they commit any error, because the disparity is quite justifiable. We must hence consider this

[10] Cf. Aristotle, *Nicomachean Ethics*, I. 3. 1094b 12 ff.; I. 7. 1098a 26 ff.; II. 1. 1103a 14 ff.; V. 10. 1137b 18 ff.; VI. 5. 1140a 32 ff.

possibility. I shall refrain from the familiar kind of invidious statements to the effect that what motivates this disparity of approaches is that moral philosophers have become infected with the ethical relativism and skepticism which marks our age, and that they exhibit the "scientism" which has faith only in the norms or criteria of science. I think we can get at the grounds they would adduce in justification of their disparate approach if we focus on the analogy which I have used to bring out the disparity in question. For what I have been saying is that in basing their theories about ethics nonrestrictively on the common denominators of the meanings of ethical terms as used by missionaries, cannibals, democrats, and Nazis, the meta-ethicists have been doing the same sort of thing as if philosophers were to base their theories about science nonrestrictively on the common denominators of the meanings of the words "science," "know," "true," and the like, as used by neurologists, Christian Scientists, astronomers, and astrologists. Thus I have been assuming an analogy which runs somewhat as follows: as Christian Science is to neurology, so is Nazi ethics to democratic ethics. But this analogy, it may be held, is open to attack on many grounds.

(1) *The nature and function of science and ethics.* Does Christian Science, and the like, constitute a "science" or "knowledge" in anything like the way in which Nazi ethics, and the like, constitutes an "ethics"? Is it not the case that the word "science" is used equivocally when applied to both Christian Science and neurology, whereas the word "ethics" is not used equivocally when applied to both Nazi ethics and Christian ethics? We may amplify this query as follows. What we mean by a "science" is a group of propositions and procedures which perform certain functions: they enable us to describe, explain, and predict phenomena. These achievements constitute what we may call a "scientific" adjustment to the world. But now Christian Science, astrology, and so forth, either do not claim to perform such functions at all, or else if they do claim to do so, they can be proven to be not even minimally successful in fulfilling these claims. On the first alternative, Christian Science, and so forth, are not even claiming to be "sciences" in the sense in which we use the term, so that the word is admittedly being used equiv-

ocally. On the second alternative, Christian Science, and so forth, fall so completely short of what is achieved by neurology and other sciences that, if the word "science" is applied to both kinds, then it is only in the sense in which one word applies to the two opposite extremes of what is not even a continuum. In either case, the failure of Christian Science, and so forth, to *be* "scientific" is conclusively demonstrable. Consequently, there is clear justification for the philosopher of science's normative restriction of the word "science" to neurology, and so forth, as against Christian Science, and so forth.

Now contrast this with the situation in regard to ethics. What we mean by an "ethics" is a body of rules, ideals, and practices which enable us to get along with ourselves and with other people. An ethics, in other words, functions as a way of life, as is suggested by the etymological connection of the words "ethics" and "morality" with habits and customs. Now the fact is that there are and have been many different ethics in the sense of functioning, viable rules or ways of life. The Dobuans, the Trobriand Islanders, the Middletowners all have their own ethics, and even though we may not approve of them, they nevertheless do function as kinds of ethics in a sense in which Christian Science and astrology do not function as kinds of science. Hence there are far more kinds of "ethical" adjustments to the world than of "scientific" adjustments to the world. The reason for this is that far more latitude is possible in respect to ways of life than in respect to ways of knowing the world. As Feigl puts it, "The validating principles of deductive and inductive logic do not seem at all to have any plausible alternatives or competitors."[11] And the reason for this, in turn, is that there is a kind of cognitive compellingness found in the bases of deductive and inductive logic—consistency on the one hand, sense perception on the other—which is not similarly to be found in the bases of any one ethics. To be sure, philosophers and logicians may have different theories *about* science, sense perception, and logic; but logic and science themselves are basically uniform in their underlying norms in a way

[11] H. Feigl, "Validation and Vindication: An Analysis of the Nature and Limits of Ethical Arguments," in *Readings in Ethical Theory*, ed. by W. Sellars and J. Hospers (New York, 1952), p. 676.

which is not the case in regard to the diversities of ethics. Consequently, the philosopher of science is justified in confining his purview to the norms of genuine science, while the meta-ethicist would not be similarly justified in confining his purview to just one kind of ethics and ignoring the large number of viable alternative ethics.

(2) *Knowledge of science and knowledge of ethics.* There are experts in science but not in ethics, institutes and academies for scientific research but not for ethical research. As Plato pointed out, all men consider themselves experts when it comes to moral virtue but not when it comes to scientific knowledge. Hence the meta-ethicists show both their realism and their democratic bent when they go for their materials to ethical discussions in daily life. The philosopher of science, by the same token, is justified in regarding science as a non-ordinary subject developed by experts, and in confining himself to what meets the norms in question.

(3) *Beliefs or opinions about science and ethics.* Men differ far more in their beliefs or opinions about what is ethically good or right than about what is scientifically valid or true. The Nazis and the Soviets, for all their differences from the Western democracies in their ethical beliefs, still had, and have, very largely the same faith in science, and in the same kind of science, as is found in the Western democracies. Hence, on this dialectical ground of universality of recognition, too, the philosopher of science is justified in adopting a normative approach to science, while the meta-ethicist is justified in taking account of the great diversity in ethics and not trying to single out only one ethic for favored attention.

(4) *Extent and resolubility of disagreements in science and ethics.* Even if one assumes a common ethical framework such as that of liberal democracy, ethical disagreements still occur within that framework which are not resoluble by cognitive means as they are within science. Men may uphold all the ethical values of liberal democracy and still differ sharply on such issues as capital punishment and euthanasia, for the issues are not resoluble solely by reference to those values, unless we define the latter in a question-begging way so as to include one or another of the disputed alternatives. Consequently, the model of ethical

argument cannot be assimilated very closely to the model of scientific argument; the field of ethics is far more pluralistic and indeterminate than that of science, in that in ethics there are many more *kinds* of objects of argument, which cannot be subsumed under a single principle or set of principles. The meta-ethicist must take account of this pluralism.

It is now time to consider the merits of these defenses of the disparity between the normative approach to science and the positive approach to ethics. I think that many points in these defenses are sound. But they nevertheless beg the question at issue. For they all proceed by way of a normative definition of "science" and a positive definition of "ethics." Moreover, the evidence they adduce for these disparate definitions could easily be balanced or outweighed by counter-evidence for defining both "science" and "ethics" as normative, and even for defining "science" in a positive way and "ethics" in a normative way.

(1) Consider, for example, the question of the "nature and function" of science and ethics. It is true that if one defines "science" by reference to empirical description, explanation, and prediction, then a normative, restrictive approach to science is justified, and Christian Science, and so forth, are only equivocally to be called "sciences." But by the same token, if one defines "ethics" by reference to impartial reconciliation of conflicting interests and mutuality of consideration, then a normative, restrictive approach to ethics would also be justified, and Nazi ethics, and so forth, could only equivocally be called "ethics." Moreover, one might counterbalance this normative approach to "ethics" with a positive approach to "science," such that Christian Science, phrenology, and astrology would all be kinds of "science" in a nonequivocal sense. For all of these claim to provide a scientific adjustment to the world in the sense that they enable men to understand the underlying realities of the world. All of them, that is, make cognitive claims, in that they claim to know and to present true general propositions about the world. As was indicated above, the meanings of such terms as "know" and "true" are not entirely the same in all these cases. But they have at least as much generic similarity as have words like "good" and "ought" when used by Nazis, democrats, missionaries, and

cannibals. Where they differ is on the contents of the propositions which they declare to be true, and on the epistemological questions of how to validate these claims. Similarly, Nazi and other kinds of ethics claim to tell us what good men ought to be like and how they ought to act, but they differ on the content of these "goods" and "oughts," and to a certain extent at least on the ways in which one should go about ascertaining who is good and doing what one ought. Hence, whether one puts the differences between the Nazis and the democrats in terms of the specific meanings of "good," "ought," and so forth, or in terms of the criteria of application for these words, the same factors could be used to explicate the differences between the Christian Scientists and the neurologists with regard to "true," "know," and so forth. And just as the Nazis, democrats, and so forth, may be said to agree on the *generic* meanings of "good," "ought," and so forth, so the Christian Scientists, neurologists, and so forth, agree on the *generic* meanings of "true," "know," and so forth.

If the nonrestrictive approach to "ethics" be justified on the ground of the plurality of the ways of "getting along" with people, a nonrestrictive approach to "science" may be similarly justified on the ground of the plurality of the ways in which men have actually made "cognitive" adjustments to the world. The existence of tribal medicine-men and the many other superstitions in the history of mankind, as well as the many fundamental disagreements among philosophers, scientists, and others claiming to present knowledge of reality, show that the sphere of the cognitive is at least as involved in sharp contrasts of basic content and method as is the sphere of the ethical. In this respect human nature is just as plastic, and environment and history just as productive of diversities, in the sphere of the cognitive as in the sphere of the ethical. On the other hand, just as our language recognizes a perfectly clear sense in which some "sciences," such as Christian Science, are not scientific, so too it recognizes a clear sense in which some "ethics," such as that of the Dobuans or the Nazis, are not ethical. And just as the criteria involved in the first statement can be specified in such terms as predictive fruitfulness and empirical testability, with whose explication the philosophy of science is concerned, so the criteria involved in the

second case can be specified in such terms as mutual consideration and impartiality, with whose explication the philosophy of ethics should be and traditionally has been concerned.[12]

As for the "compellingness" of basic principles of science and of ethics, this is to be interpreted either logically or empirically. If it is interpreted logically it begs the question, for what is at issue *is* the question of what is the right or cognitively cogent logic of science. And if it is interpreted empirically, as referring to what men have in fact found to be compelling, there is a good deal of evidence to suggest that ideals like the golden rule have seemed to men to be no less cogent ethically than principles like that of noncontradiction and experimentalism have seemed cogent in the logic of science.

(2) The fact that there are no recognized experts in ethics as there are in science is a consequence of the fact that ethics is a practical matter affecting everybody, whereas science is a more specialized matter. Yet this does not mean that standards of comparative value or excellence are not as possible in ethics as in science, nor does it mean that such standards cannot be recognized and explicated by philosophers and others.

(3) On the other hand, to what extent are the diversities of men's opinions about science and ethics relevant to our problem? This dialectical level provides a prime field for the disparate approach. Feigl, for example, writes that the "basic criteria" of science "have attained practically universal consent. It is only too tempting to hope for a similarly universal code of morals." But he rules this out because of such factors as "the stark realities of group and culture-centered ethical standards."[13] Now if Feigl is here making a sociological statement about the greater degree

[12] This point would also be relevant against any attempt to justify the disparity of approaches to science and ethics on the ground that "science," unlike "ethics," is always an "achievement word" (cf. G. Ryle, *Concept of Mind*, London, 1949, pp. 133-134, 152, on "know"). For in phrases like "medieval science" or "Nazi racial science" we use the word "science" to refer to claims to cognitive adequacy which are not fulfilled or successful, so that "science" is not always an achievement word. Moreover, can we get a sound view of the logical relations between science and morally good ethics by concentrating only on the features which the latter shares with all other "ethics"?

[13] Feigl, *op. cit.*, pp. 676-677.

to which men all over the world "consent" to scientific as against ethical criteria, he adduces no statistical or other evidence in support of this, nor does he indicate the relevance of such sociological or rhetorical considerations to the question of the validity or logical status of scientific and ethical norms. The meaning of his statement seems rather to be that scientifically-minded persons all over the world accept the same scientific criteria, while men and groups without qualification accept different ethical criteria. But it could just as well be held that ethical men all over the world accept the same ethical criteria, while men and groups without qualification accept many different kinds of superstition and other unscientific criteria. Would this latter fact invalidate the universal *validity* of scientific criteria?

(4) One may grant that the field of ethics is more indeterminate and pluralistic in respect of disputed issues and principles than is science and still point out that these issues and principles may on closer scrutiny turn out to be more unified than we think. In this respect the drive toward unity of science is matched not only by the increasing communication and homogeneity of ethical standards among the peoples of the world, but also by the traditional concern of philosophers, in opposition to cultural relativism, to find unifying principles under which to organize and interrelate man's ethical ideals.

V. Conclusion: Philosophic Ethics as a Normative Discipline

The upshot of these considerations is that, to a large extent at least, the disparity between the normative approach to science and the positive approach to ethics is not justified. I do not pretend to have exhausted every possible argument in favor of the disparity, nor do I deny that the arguments presented carry some plausibility. Yet the arguments against the disparity have by far the greater weight. What follows from this is not that ethics ought to be "scientific" in the sense of being deduced from science or of using the same methods as science. The burden of the argument has been rather that there should be a parity in the approaches which philosophers take to ethics and to science insofar as those approaches delimit or recognize the respective fields as

being constituted by the norms relevant to each. Initially I have argued for this parity insofar as it has concerned the explicit or implicit comparisons between ethics and science which have been used by philosophers to deal with the cognitive-noncognitive issue about the meaning of ethical terms. But in addition I have held that even apart from this issue, the disparity with its positivist approach to ethics makes it impossible to do justice to distinctive features of ethics in the way in which the normative approach in philosophy of science does justice to science. From this it follows that the philosopher of ethics, like the philosopher of science, must begin with the recognition that the field about which he is to philosophize is constituted by certain basic norms, and that his job is to explicate their content and how they are exhibited in that field.

But which norms? And on what ground do we select some norms rather than others? Does not our insistence on beginning with ethical norms open the door to dogmatism, parochialism, and worse?

There is not space here to go into the content and logic of the normative approach to ethics that I would substitute for the positive approach; I hope to deal with this elsewhere. The following considerations are relevant. There can be no absolute justification of ethical norms any more than of scientific ones; for any attempt to justify the norms in either field will sooner or later adduce criteria which assume the very norms in question. But on the other hand, just as we do not therefore accept skepticism in regard to science, so too we need not accept skepticism in regard to ethics. The areas to which one should look for ethical norms are multiple; I shall here indicate only one of them, which is in line with some of the illustrations used in this paper. The development of the ideals of modern liberal democracy, involving in particular certain methods of resolving conflicts of interest, is just as compelling for the field of ethics as is the development of the hypothetico-deductive-experimental method for the field of science. The moral philosopher who attempted to reject the former ideals in favor of their contraries would be just as philosophically suspect as would the philosopher of science who insisted on treating the cognitive methods of nuclear physicists and tribal medicine-men

as if these were scientifically on a par. This does not mean that the moral philosopher should accept these ideals "unquestioningly" any more than the philosopher of science unquestioningly accepts the canons of scientific method. But the philosopher in each of these fields must regard the norms in each case as providing the subject matter which he is to subject to critical analysis. Such analysis clarifies the meaning or content of the norms and thereby sometimes suggests better ways of formulating or interpreting them.

The removal of the unjustified disparity between the approaches of the philosopher of science and the moral philosopher should help to return the latter to his traditional role of clarifying and criticizing men's moral ideals within the context of ongoing moral practices and institutions. The result should be not only the avoidance of a large measure of noncognitivism, but also the reinstatement of philosophical ethics as a normative discipline.

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ABSTRACT ENTITIES

I

THE two sentences "Look at the color of that apple!" and "Look at that apple!" seem to command us to look at different things and to look at the same thing.[1] Yet this circumstance ordinarily occasions no confusion. We say that one may look at the apple without specifically looking at its color, and that one is looking at the color rather than the apple when his attention is directed specifically to its color. This difference in the direction of attention is all we need to account for the fact that the two sentences seem to command us to look at different things and to look at the same thing. There is no need to claim that the phrase "the color of that apple" singles out for attention an entity which exists apart from the apple. The difference between the apple and its color is a difference to be realized in acts of attention and not in the existence of separate entities singled out for attention. The question of two kinds of entities, abstract and non-abstract, simply does not arise in this context. There is but one entity, the colored apple, though it is an entity to which attention can be directed in various ways.

When an abstract term such as "color" is used as it is in the above example, I call it "a term of *directive abstraction*." Its use is to direct attention rather than to single out something for attention. The phrase "that apple" in both sentences singles out an object for attention, while the phrase "the color of" in the first sentence directs attention—specifies how one is to pay attention—to the object singled out for attention. It is sometimes unnecessary to have a phrase which singles out the object. In a context where it is obvious that two people are both looking at the same apple, one of them may say merely, "Look at that color!" In this case

[1] Several of the points mentioned in this paper, especially the distinction between directive and hypostatic abstraction explained briefly in Section I, have been treated more fully in my "Abstract Terms," *Philosophical Review*, LXVIII (1959), 281-302. The present paper is intended as a sequel, but it may be read independently.

the person is not singling out another object for attention; rather, he is directing the other person to pay attention in a certain way to the object both have already singled out for attention.

Most if not all abstract terms are frequently used in directive abstraction, and it is essential to keep this use distinct from another very common one. Instead of directing a person to look simply at the color of an apple, we of course might direct him to look at its specific color. We might use as phrases of directive abstraction "the redness of that apple," "the red color of that apple," or even "the red of that apple." But we would not use "the color red of that apple." "The color red" is a phrase we use when we wish to single out the color itself for attention rather than to direct attention to the color of an object. And in contexts where we use this phrase, there is the question of abstract as opposed to nonabstract entities. We single out the color red for attention by reorienting our language; we transform phrases which specify a way of paying attention into phrases which single out an object for attention. Instead of saying "the red color of" we say simply "the color red"; instead of directing attention to an object in the way that we call "noticing its color," we profess to single out an object for attention. When an abstract term such as "color" is used in this way, I call it "a term of *hypostatic abstraction*," since in this use we appear to create (to hypostatize) objects. These created objects are what I mean by "abstract entities."

While the contrast between the phrases "the red color of" and "the color red" indicates that "color" is not used in the same way in the two cases, there is by no means always such a clear indication of the difference between directive and hypostatic abstraction. Often the same sentence, depending on its context, may constitute a use of abstract terms in one way or the other. When a person says, for example, that wisdom is preferable to courage, does he mean only that a wise man is preferable to a courageous one, that the wisdom of men is preferable to their courage? Or does he mean that when virtues as abstract entities, considered without reference to whether men are ever virtuous, are ordered in a scale of preference, wisdom stands higher than courage? We can answer these questions only when we know something of the context in which the sentence occurs. This dependence on context

makes it easy to confuse the two kinds of abstraction, and in particular to draw the conclusion that with every use of an abstract term we run into the question of abstract entities. Contrary to this common conclusion, I propose to consider abstract entities, starting from the principle that the question of such entities is relevant only when terms are used in hypostatic abstraction.

My concern, then, is to find peculiarities in hypostatic abstraction which will help to elucidate the notion of an abstract entity. One such peculiarity seems to lie in our use of the verbs "to be" and "to exist" when we are speaking about entities singled out for attention by terms of hypostatic abstraction. Suppose that we have a row of color patches arranged according to some principle for ordering colors, such as increasing degrees of brightness, darkness, or warmth. Suppose further that each patch after the first differs from its predecessor in visibly the same degree, with one exception. At some place in the row there is an obvious gap, a patch which differs from its predecessor in a visibly greater degree. We might expect a person noticing the gap to remark, "There is a color missing."

Now if one were to challenge this statement, what would be adequate for its defense? The mere perception of the gap is not enough; the gap by itself is not evidence for the existence of something which belongs in it. In addition to the gap we need the assumption that the order of the patches is determined by a given rule or principle. But then suppose that the challenger grants this assumption and admits he perceives the gap, yet denies that a color is missing on the grounds that no one has ever seen a color which would fill the gap. "How can you assert that a color is missing," he may ask, "when you have no evidence for its existence?" In answer to this objection we may point out that someone's having seen the color is not the sort of evidence for existence that is relevant. Our assertion that a color is missing presupposes that the row of patches is a representation of a definite color series, the series defined by the principle of order which we have assumed applies to the row. We can show that it follows logically from this principle that there is a color in the series which is missing in our representation of the series. The relevant

evidence for existence is this logical deduction and not the fact that someone has seen the color.

The point, in other words, is that in asserting that a color is missing we are using "color" as a term of hypostatic abstraction. We are singling out for attention the colors in a series defined by a principle of order, and our assertion is simply that at least one of these colors is missing in a particular representation of this series. On the other hand, we would be using "color" as a term of directive abstraction if we wished to direct attention to the fact that a patch of a certain color, say red, is missing in a row of paint samples. The sentence "There is a color missing" would then be at best a rather cryptic way of saying what we intend, and we are much more likely to say, "A red sample is missing." We would not say "The color red is missing," since we do not want to single out colors as objects for attention but to call attention to the row of patches in a way that brings out the fact that a red sample is missing. Our remark presupposes the existence of red paint, but the relevant evidence for existence in this case is clearly not a logical deduction.

This example suggests that at least part of what we do when we create entities by hypostatic abstraction is to define a system or an order by means of principles. For such an entity, to be is to be a member of—to have a place in—the system, so that any question about its existence can be settled by the principles which determine the system. But we must remember that this formula is relevant only in contexts where we claim to be talking about abstract entities. Suppose a detective deduces from a set of assumptions that there is a single individual who left certain fingerprints, made certain footmarks, and committed a certain murder. We may say that such an individual is a member of the logical system constituted by the detective's assumptions and rules of deduction, yet we would not say that the individual so determined is what the detective claims to be talking about. If it were established empirically that one individual left the fingerprints and another the footmarks, we would consider it absurd for the detective to say, "Never mind, in the logical system established by my hypothesis there is a single individual who does both." The fact that abstract entities are created by the detective's

deductions is in this context utterly irrelevant to what he is talking about—to what he is trying to establish by means of his deductions. I am interested, on the contrary, in those contexts where we seem, by our use of terms of hypostatic abstraction, to be talking about abstract entities. I want to explore the suggestion that in such contexts we are talking about entities the existence of which is established solely by membership in a logical system.

II

Instead of saying that we are considering something as an abstract entity, we are far more likely to say that we are considering it in the abstract. A fairly common alternative to the latter phrase is the expression "itself" or "in itself." We say that justice in the abstract is justice itself or in itself as opposed to justice in actual court decisions, and that numbers in the abstract are numbers themselves or in themselves as opposed to actual numbers of objects. On the other hand, when we speak about just decisions in the courts and about numbers of objects, we use "justice" and "number" as terms of directive abstraction. We direct one to pay attention to the conduct of a court, noticing in particular the justice of its decisions; and to pay attention to a group of objects, noticing in particular their number. Justice and numbers thus are inseparable from the circumstances under which they come to our notice. In this context it would be senseless to ask that we remove the justice from the conduct of the court or the number from the group of objects, in order that these things might stand out more clearly and be more noticeable. The whole point of our directive abstraction is to bring justice and number to notice *as they are* in the actions and objects we single out for attention. The removal of justice and number from the particular circumstances (assuming that it makes sense to talk of such a thing) would utterly defeat our purpose.

In contrast to the above, when we single out objects for attention by hypostatic abstraction, the separation of objects like justice and number from particular circumstances is precisely our aim. By "in itself" we mean not in something else, not in particular circumstances. Yet we hardly mean wholly apart

from anything else, since we do not mean to limit our discourse about something in the abstract to repeated utterances of the same word. We are not content to say that justice in the abstract is justice, period. We say that it is one of several virtues, interests, or values and that it is related to other things of this sort in various ways. Far from considering it wholly apart from anything else, we consider justice in relation to other abstract entities as soon as we commence to consider it in the abstract. We first give it some location in a system of virtues or values, and we then claim to be elucidating justice in itself when we are showing its systematic interrelations with other virtues or values. We thus can single out justice in the abstract as an object for attention only when we have a system of order in which justice occupies a specific place.[2]

III

In opposition to the view that the existence of abstract entities is always to be decided solely by what we can establish in a logical system, Quine offers the example "the ratio of the number of centaurs to the number of unicorns." He contends, "If there were such a ratio, it would be an abstract entity, viz., a number. Yet it is only by studying nature that we conclude that the number of centaurs and the number of unicorns is 0 and hence that there is no such ratio."[3]

But do we establish the nonexistence of the ratio by studying nature? Suppose that one admits the nonexistence of unicorns and centaurs, but then announces that their ratio is 0/0. Further study of nature at this point will not help. We simply inform the person that it follows from the arithmetic properties of 0 and the

[2] Along with "in the abstract" and "in itself" we also have "in the ideal," "in theory," and "in principle." The last two phrases bring out explicitly the presupposition of a system, but "in the ideal" as an alternative for these phrases must not be confused with phrases which use "ideal" as an adjective. "He is the ideal father" usually means merely that the man intended possesses all the characteristics which the speaker would like to see in a father. On the other hand, it might mean that the man is a near perfect embodiment of ideal fatherhood; and in this case the speaker is presupposing a logical system or theory in which the entity called "fatherhood" is defined.

[3] W. V. Quine, *From a Logical Point of View* (Cambridge, Mass., 1953), p. 3, n.

definition of a ratio that there is no such ratio as he is claiming. The nonexistence of the abstract entity is determined by what we establish within a logical system; the study of nature is relevant only when we seek to identify abstract entities, not when we seek to establish their existence or nonexistence. Of course, one may say that the identification of the numbers is relevant to establishing the nonexistence of the ratio because unless we know whether or not the number of unicorns is 0 and the number of centaurs is 0, we cannot know whether or not there is such a ratio. But this is like saying that unless you can identify the number I am referring to you cannot know whether or not there is such an entity as the third prime factor of this number. If I tell you straightaway that the number is twice ten you can make the identification without observing nature and proceed at once to establish the nonexistence of the entity in question. But if I tell you only that the number is the number of trees in my garden, you may have to turn to an observation of nature before you can settle the existence question. Again, if I say only that it is the number of books on my desk or that it is the number encoded as "ab" in the number code list in the top drawer of my desk, you may have to begin with an empirical observation, though not an observation of nature. In any case, the observation is relevant to the identification question "What is the number of. . .?" or ". . . the number which. . .?" as opposed to the existence question "Is there an entity which. . .?" A sentence like "Is there an entity which is the third prime factor of. . .?" will or will not express a question calling for empirical observation, depending on the word or phrase which is used to fill the blank at the end. But the observation will be relevant only to a secondary question which arises because of the peculiar way in which the primary question has been expressed; it will not be relevant to the primary question itself, since this question does not have to be expressed in a way that calls for such observation before it can be answered.

In this sense of "relevance," empirical observation can be made relevant to the answering of any question. One can always use a phrase like "the third equation on the second page of . . ." and sometimes this provides the most convenient and natural way of putting a question about the existence of mathematical entities.

But then we do not mean that the observation of what is written on a given page is indispensable to establishing the existence or nonexistence of the entities in question. We mean only that identification of the equation which on this occasion is referred to by the phrase "the third equation . . . etc." is indispensable. Similarly, we do not mean that the observations which indicate that there are no unicorns and no centaurs are indispensable to establishing the nonexistence of a ratio. We mean only that identification of the number which on a particular occasion is referred to by the phrases "the number of unicorns" and "the number of centaurs" is relevant. This is not to deny that a phrase like "the number of . . ." is commonly used in conveying information about empirically observed objects. We have already remarked that "number" is used as a term of directive as well as hypostatic abstraction and we may achieve further understanding of the examples just mentioned when we note that "number" can have both these uses at once.

The sentence "Look at the number of books piled on that desk" directs attention in a certain way to the pile of books. This directing of attention may lead one to count the books and not merely to notice that there are quite a lot of them. When a person counts the books he determines empirically a numerical value for the expression "the number of books piled on that desk"; he determines which number in particular this expression as a phrase of hypostatic abstraction refers to, but he does not thereby establish the existence of this number as an abstract entity. It follows from the fact that one can count the books that there are some of them, but this is irrelevant to the question of whether we can establish within our number system that there is a number to which, as a phrase of hypostatic abstraction, the expression "the number of books . . ." refers.

Suppose that I remark with intended irony, "You can see that he's been studying hard. Look at the number of books piled on his desk." You look and see that his desk is bare. You fail to catch my irony and you retort, "But there isn't a number of books on his desk. There are no books there at all." I answer, "Oh, yes, the number is zero." You may protest that zero is not a number, meaning that "number" as a term of directive abstraction is not

used to direct attention to the absence of something. Your point may be well taken if we ignore my irony. But your point has nothing to do with the question of whether there is an abstract entity, the number zero. You might urge a similar point if there were only two books on his desk, since one might claim that in this context two is not a number but only a few.

Situations of this sort are not likely to create confusion. But if we take a phrase like "the number of books on his desk" out of context and ask what it means, we may forget that it can perform two very different functions at once. Confusion is inevitable if we try to combine these two into a single function. We note that as a term of hypostatic abstraction the phrase refers to a particular number, an abstract entity; and we note that as a term of directive abstraction the phrase calls attention to a particular pile of books. In an effort to combine the two, we are led to think of the abstract entity as inseparable from the pile of books though we see that this will not do. We do not want to say that in destroying the pile of books we are destroying the abstract entity. The existence of the latter then seems mysterious and we wish we could establish its existence as easily as we establish the existence of the pile of books.

In order to escape from our difficulty we must recognize (1) that the abstract entity is something apart from the pile of books, something the existence of which we can establish without reference to the pile of books; and (2) that we can say anything we want to say about the books without referring to an abstract entity. The sentence "There are twenty books in that pile" does not have to be taken as referring to anything other than the pile of books and a sequence of number words. One notices that there are twenty books simply by running his eyes over the pile and repeating to himself the number words from "one" to "twenty." The fact that in order to make this observation one must have memorized a sequence of number words does not mean that one must refer to abstract entities. Even a remark like "That pile can be made into two piles of ten books each" does not have to be taken as referring to anything other than the pile of books and sequences of number words. One may notice this further fact by running his eyes over the pile and repeating twice the sequence

of number words from "one" to "ten." The important thing in this second observation is that one sees that he gets the same result by going through a certain sequence of number words twice or by going through a certain other sequence once, and in order to see this it is not necessary to look at the books. One may write down on paper the sequence of number words from "one" to "twenty," and then just beneath it write the sequence from "one" to "ten" twice. From this pencil-and-paper observation plus the fact that "twenty" is the last number word used in checking off the books in the pile, one gains the information that the pile can be divided into two piles of ten books each. Such pencil-and-paper observation or its equivalent (for example, a calculating device) may be indispensable in more complicated cases, as when we observe a body in accelerated motion and want to determine its instantaneous velocity at a particular time.

I am not trying here to argue for a particular philosophy of mathematics. I want only to urge the point that our use of number words in making statements about empirically observed entities does not have to be construed as making a reference to abstract entities. It may very well be the case that if men at some time had not construed number words as making a reference to abstract entities, mathematics as we know it today would never have developed. But this is another point, and one I am not concerned to argue. We refer to abstract entities when we take our number words as names rather than as mere vocables to be correlated with discriminations we make in directive abstraction while observing empirical objects like piles of books or bodies in motion. While at first we may speak of no new entities beyond those which we take as named by the number words we have already learned to use in our observations, as we become familiar with techniques of mathematics we add new number words to our vocabulary and hence speak of further entities. When we have the number word "zero" we can say that no books are a number of books, even though with "number" used only in directive abstraction this statement would ordinarily be taken as a flat contradiction. We thus come to see that any question about the existence of entities named by our number words is to be settled by what we establish within our number system and not by anything we observe in the

entities to which we direct attention when we use "number" as a term of directive abstraction. Once we accept our number words as referring to abstract entities, we can regard the subject term in a sentence like "The number of books is twenty" as performing a dual function. As a term of hypostatic abstraction it refers to an entity in our number system, but it can also be taken as a term specifying something to be noticed about a particular pile of books. We illustrate these two functions when we note that the entity in our number system is twice ten and confidently assert that the books can be divided into two piles of ten books each. We thus have knowledge of abstract entities where in our previous context we had the result of pencil-and-paper observations.

The further point I want to urge, then, is that when we claim to be talking about abstract mathematical entities we never decide whether there are such entities by carrying on the sort of thing we usually call "empirical observation." We always decide the question solely by what we can establish within a mathematical system. This point stands whether one holds on philosophic grounds that in mathematics we must talk about abstract entities or whether one insists that we need only talk about results of pencil-and-paper observations. If we do talk about abstract entities, the only thing we mean by saying that they exist is that they have a place in our system.

IV

These two points regarding mathematical entities hold, *mutatis mutandis*, when the abstract entities are nonmathematical. In the example of the missing color in a series there was occasion to refer to abstract entities only because the missing color was not identified as the color of an object already observed. But this example presents a rather unusual case, since in most of our talk about colors we can say that we are talking about the colors of objects already observed. An utterance of "Red is darker than orange," for example, seems easily understood as making a statement contrasting the colors of certain objects. But suppose that a person were to claim that in uttering this sentence he is making a statement about the colors themselves and is not referring to colored

objects. "I am not saying," the person may protest, "that a red object compared with an orange one will always look darker, though this is usually the case. I am saying only that red is a darker color than orange." If we grant this person's claim, what entities shall we say he is talking about? Quine has suggested that if we regard all red surfaces as constituting a single entity extended in space and time, we can say that this entity is the color red.[4] If we accept this suggestion we have to say that it makes sense to ask of red and orange which is larger, which has more parts, and which is older. Yet these are hardly appropriate contrasts when one claims to be talking about colors themselves. The appropriate contrasts in this case are limited to those which are proper color contrasts, such as darker, brighter, warmer, stronger, more vivid. This suggests, contrary to Quine, that a person is talking about colors themselves when he claims to refer to entities which contrast only as colors.

But suppose a person says that the color of the cushions is darker than the color of the sofa, and claims that the only entities he is referring to are the cushions and the sofa. In this case the contrast in question is not a proper color contrast. It is a contrast between colored objects rather than between colors themselves. Instead of saying that the color of the cushions is darker than the color of the sofa, the person could just as well say that the cushions are darker in color than the sofa. With the statement in this second form, it is clear that the entities referred to are the cushions and the sofa and that the contrast between them is that of being darker in color and not the proper color contrast of being darker. The addition of the phrase "in color" does not make sense when one is talking about colors themselves. Red is not darker in color than orange, it is simply darker than orange. If we want to add the word "color" we say that red is a darker color than orange. One might say that the cushions are a darker color than the sofa, but this would obviously be elliptical for "the cushions *have* or *are of* a darker color than the sofa." The word "color" is here used only in directive abstraction and there is no question of entities apart from the cushions and the sofa.

[4] Cf. *op. cit.*, p. 69.

We may thus distinguish two functions which can be performed by a phrase like "the color of the sofa," analogous to the two functions performed by a phrase like "the number of books." But in the case of colors the distinction works in a rather different way. We generally take the reference of number words as fixed by the abstract entities to which these words refer as terms of hypostatic abstraction. If you tell me that there are twenty books on the table, and I assume that you have counted correctly and that you are correctly using the number word "twenty" as a term of directive abstraction to call my attention to something about the pile of books which I would notice if I looked at the pile in a certain way, then I would readily answer various questions about the number of books on the table without bothering to look at the books myself. I would say that their number is twice ten, greater than nineteen, a multiple of five, and the like. The questions I would be answering are of the sort we might call "proper number questions," questions we ask about numbers themselves. On the other hand, if you told me that there was a red book on the table, even though I assumed that you had observed correctly and that you were correctly using the color word "red" to direct my attention to something about the book which I would notice if I looked at it, I still would not be ready to answer many proper color questions about the color of the book without first looking at the book myself. I would want to know the shade of red before I said much about how the color of the book contrasted with other colors, and I cannot determine this shade from my knowledge of the ordinary use of "red." The two of us might have agreed to fix the reference of "red" for our discussion either as the color of some particular red object or as the color occurring at a specific place in a color series, but this would of course be giving the word "red" a special use.

Yet there are a few questions which I would answer from my knowledge of the ordinary use of "red" without bothering to look at the book. I would say, for example, that the color of the book is darker than yellow and warmer than blue, because I assume that any shade of red contrasts in this way with any shade of yellow or blue. In a context limited to these few questions, a phrase like "the color of that book" may thus serve both to name an abstract

entity called "the color red" and to direct attention to a particular book. But the questions in this case by no means include all questions about proper color contrasts. I would not want to say, for example, without looking at the red book, that its color is warmer than orange. Our ordinary use of color words does not fix the reference of these words to abstract entities sufficiently to enable us to answer many questions of color contrast. In our ordinary discourse we do not presuppose a single system of colors (analogous to our system of numbers) within which we can locate the color of any given object and from its location determine all its contrasts with other colors. We are more inclined to think of color words as having their reference fixed by directive than by hypostatic abstraction, but we nonetheless recognize a few color contrasts, such as that red is darker than orange, which we take as sufficiently general to hold for any use of the color words in question in directive abstraction. In stating these contrasts we may claim to be talking about colors themselves, but even so we are not likely to say that we are talking about entities the existence of which is determined solely by the principles defining a color system. We tend to think of the colors as familiar objects and to take their existence as settled simply by the fact that we see them.

It is only when we are pressed to specify exactly what we mean by colors themselves apart from colored objects that the existence of colors begins to seem mysterious. We recognize that by "colors themselves" we do not mean objects which contrast as being larger, older, or in any other way that is not a proper color contrast. While we speak of creating, destroying, adding, or taking away a bit of red, we do not mean in such cases to be referring to the color red itself but to the physical act of altering in a certain way the color of an object. When we say, on the other hand, that one color is darker than another, it is not always so easy to specify what we are talking about. In some contexts the answer might be simply that we are talking about objects as differing only in color, but in other cases we would want to say that we mean only the colors without reference to the objects. If we say the latter, one may ask us questions like the following. "Does the phrase 'without reference to the objects' mean that you suppose the colors to

exist independently of the objects—that you want to claim that the colors still exist even though the objects are destroyed? Or does the phrase mean merely that you are considering only the colors of objects and are ignoring all other properties of objects? If the latter is what you are saying, how can you distinguish between talking about objects as differing only in color and talking about colors themselves?"

These are not questions which ordinarily arise in our discourse about colors. They are philosophic questions and if we try to answer them directly we shall have to support our answer by philosophic arguments. We cannot give answers and simply claim that anyone who understands what it means to say that one color is darker than another will recognize the correctness of our answers. A person may point to two colored objects and say, "The contrast between this and that color illustrates what I mean by saying that one color is darker than another." We hardly want to claim that this person does not understand what it means to say that one color is darker than another, yet the person has said nothing which will decide our philosophic questions about the existence of colors themselves in relation to the existence of colored objects. If we press these questions of existence, the person may very well claim that for him such questions do not arise. "There are the colors. Look at them!" he may say. "Your odd questions about colors apart from colored objects have nothing to do with what I mean by saying that one color is darker than another."

The example of the missing color in a series supplies a context in which a question about the existence of colors cannot be dismissed in this fashion. When a person in this context says that *there is* a missing color, he is clearly using "there is" existentially rather than demonstratively. He is explicitly asserting that a color exists and is not merely saying something about the color and presupposing that it exists; he cannot say, "There is the color. Look at it!" and claim that questions about the existence of colors are irrelevant to his assertion. In so far as the reply that a color exists when it has a place in a system of colors is the proper answer in this context, we do not have to support the answer by philosophic arguments. We may claim that anyone who understands

what it means to say in this case that there is a color which is missing will recognize the correctness of the answer. But then with this reply we have answered the philosophic questions which we raised previously. The phrase "without reference to the objects" means in this context that we are using color words to refer to entities defined by a system of colors and not to direct attention to colored objects. We are doing the latter when we talk about objects as differing only in color, as when we say that the orange rather than the red cushions would look well on the blue sofa. But when we say that one color is darker than another we may mean only to contrast two entities in a color system.

This example of the missing color was originally selected as affording a clear case of "color" used as a term of hypostatic abstraction. It should now be noticed that in those cases where it seems possible to claim that the philosophic questions do not arise because one can simply point to the colors in question, this claim is tantamount to the denial that "color" is being used as a term of hypostatic abstraction. We may use the same language whether we wish to state a proper contrast between colors themselves or to contrast objects as differing only in color. While the conditions of the context generally make clear what is being asserted, this of course is not the case when one asks in a philosophic discussion what is asserted by uttering a sentence like "Red is darker than orange." This question has one answer when the color words are taken as terms of hypostatic abstraction and quite another answer when they are taken as terms of directive abstraction. Confusion is inevitable if these two answers are not distinguished. The answer that the sentence asserts a contrast between entities in a color system—entities which exist solely as members of the system—is an adequate answer only if the color words occur as terms of hypostatic abstraction.

It has already been remarked that our ordinary color words in contrast with number words have a rather limited use as terms of hypostatic abstraction. It thus initially does not seem so plausible for colors as for numbers when we say that for an abstract entity to be is to be a member of a logical system. While the statement may hold for the example of the missing color in a series, this example seems to present an atypical use of color words. I trust

that by now it is apparent that the example is atypical only in the sense that it represents a use of color words in hypostatic rather than directive abstraction. The example is typical of color words used in the former way, and the answers it suggests for our philosophic questions should be the proper answers in any context where questions about colors as abstract entities arise.

Most other abstract words are even more limited than color words in their use as terms of hypostatic abstraction. Consider the following two cases. "The office he is seeking is the assistant secretaryship." "The job he is seeking is that of district salesman." Suppose one were to ask, "Is there such an office?" and "Is there such a job?" It seems easier to construe the first rather than the second question as a query about abstract entities. We might take the first as asking whether the system of offices defined by a particular constitution provides for (contains as one of its members) an office of the sort we would call "an assistant secretaryship." In this case we may say that the question is answered by principles defining a system, and it is then irrelevant whether the office is now, ever has been, or is going to be filled—whether there is, ever has been, or will be an assistant secretary. In this form the question seems analogous to a question about the existence of a number or of a color in a series. But we avoid reference to abstract entities if we construe the question as about certain individuals. "Are they going to appoint someone assistant secretary?" And it seems natural to take the second question in this way. "Are they going to appoint a district salesman?" We are not likely to take "the job of district salesman" as a phrase referring to something defined by a system like the system of offices provided by a constitution. But if we were to take the phrase in this way we would make it refer to an abstract entity.

Yet suppose one objects that even with this reference to the provisions of a constitution there is no reference to abstract entities. It is simply a question of what is contained in a particular document. "Does the document contain sentences which say in effect that the secretary shall have an assistant who, along with the secretary, shall have the status of an officer?" The answer to this objection is simply that there is no reason why we should not in certain contexts construe the question in this manner, and

that when we thus construe it there is clearly no reference to abstract entities. But it must be noted that there is an analogous way of construing most, if not all, questions which seem to be about abstract entities. Let the question be: "Is there a prime number between 166 and 170?" This could be a question merely about what is contained in a particular prime number table covering the interval from 166 to 170. "Does the table contain markings which say in effect that one or more of the three-digit sequences occurring between the sequences '1-6-6' and '1-7-0' can be labeled 'prime number'?" But the question does not have to be construed in this way and in most cases it would not be. We would usually take it as a question about entities determined by our number system. It is of course not so clear how we would be inclined to take the question about the secretaryship. My only point is that *if* we do take it as about abstract entities it will turn out to be about entities determined by a system and hence to conform to the formula that for an abstract entity, to be is to be a member of a system.

V

Many other examples are needed if we are to convince ourselves that this formula applies in every case where we are prepared to say there is reference to an abstract entity. The few examples that might be crowded into the space remaining in this paper would not advance us very far in this direction. It seems better to conclude with a few remarks concerning the formula itself. Specifically, I want to comment on the question, Is the formula to be taken as specifying a sense of the verbs "to be" and "to exist"—a sense which is different from the one illustrated by a sentence like "There are tigers in Africa"?

It sounds very odd to say that in one sense of "there are" there are prime numbers over a million, or that in one sense of "there are" there are tigers in Africa. Either case represents a well understood use of the verb "to be" and it seems pointless to preface either of the statements with the phrase "in one sense of." Such a phrase, however, seems appropriate when we wish to indicate that the word in question is being used in an unusual

way, especially if it is a way that conflicts with the more ordinary uses of the word, as we might say that in one sense of "color" the sound of a flute has color. I say "conflicts with the more ordinary uses" because it would generally be taken as true and as calling for no special explanation if one were to assert categorically that sounds have no color. Similarly, we might say that in one sense of "there are" there are unicorns, meaning that there are in mythology or in a particular story. The phrase "in one sense of" may serve to warn our hearer that with "there are" as it is generally used it remains true to assert categorically that there are no unicorns.

I do not wish to press the point that we should regard a sentence like "There are unicorns in mythology" as representing a special sense of "to be," in analogy with the example about "color." My point is rather that whether or not we regard such sentences in this way is irrelevant to the question about "to be" as it occurs in our formula. We obviously would not say that with "there are" as it is generally used it remains true to assert categorically that there are no prime numbers over a million. I mention the point only because philosophers have sometimes taken the claim that abstract entities exist in a special sense as asserting that they exist in the way that fictitious and mythical entities are said to exist. The absurdity of such a claim, I trust, does not need further comment. In this context our formula for the existence of abstract entities is not to be taken as specifying a sense of the verb "to be."

But now consider the following sentences. "He often comes late and *stands* in the back of the room. He always *stands* up to his obligations and it *stands* to reason he should be the one to go." These sentences may be said to represent three different senses of the intransitive verb "to stand," though clearly there is no conflict with more ordinary uses as in the preceding examples. While the three senses may be related analogically, we hardly want to say that a person who has learned to use the verb in one of these senses will surely know how to use it in the other two as well. Rather, each sense is learned independently and the perception of analogies comes, if at all, after the different uses have been learned. Similarly, the sentences "There are tigers in Africa" and "There are prime numbers over a million" may be

said to represent different senses of "there are." If we wanted to explain to someone the role of "there are" in arithmetical sentences like "There are prime numbers over a million," we might begin with a simple proof that 4 and 6 are even numbers and then remark, "Thus, we say it follows that *there are* even numbers greater than 2." We would hardly begin by telling the person that everything will be clear if he understands the role of "there are" in the sentence "There are tigers in Africa." In this context our formula for the existence of abstract entities may be said to specify a sense of "to be" which is illustrated by the arithmetical examples but not by the example of tigers in Africa.

This is not to deny that the different senses of "to be" may be related analogically; it is only to deny that to understand the role of "there are" in "There are tigers in Africa" is to understand its role in "There are prime numbers over a million." Whether or not we call these different roles different senses of "to be" is immaterial, as long as we recognize the difference. If we take the roles as the same, we may end up seeming to say that the world is populated by numbers as well as tigers. It then only makes matters worse to say that numbers exist "not really" but only "in a sense" or "in a manner of speaking," as unicorns exist not really but only in a sense. The point of our formula is then that while abstract entities are just as real as anything else, this is not to say that along with tigers they populate the world. It is simply a misunderstanding of language to claim that when we say "There are prime numbers over a million" and "There are tigers in Africa," we are saying that prime numbers and tigers have or do anything in common. Yet, to speak of different objects as populating the world is to speak of them as sharing the property of occupying space in the world, and this is clearly not a property we ascribe to prime numbers.

These remarks about senses of "to be" are of course independent of our formula and do not provide arguments for or against its acceptance. But they tend to show, I believe, that the formula does not conflict with our use of "there are," and this at least is one condition which any such formula must meet if it is to prove acceptable.

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SAMENESS AND CHANGE

CAN something change and yet remain the same thing? Many philosophers have denied that it can. Change and self-identity, they have held, are incompatible. Some have concluded, on the supposition that self-identity is a necessary condition of a thing, that things do not change—"things," that is, in the true and proper sense of the word, "real" things. Parmenides and Plato are ancient philosophers who drew this conclusion, and it has been drawn in recent times by Whitehead. Other philosophers who have held that change and self-identity are incompatible have adopted the contrary supposition about things: it is change and not sameness which is essential to them. These philosophers have then concluded that things, at least "real" things, are not self-identical from moment to moment. Heraclitus, apparently, was expressing this conclusion when he made his famous statement about stepping into rivers. And Bergson, more recently, has made a similar point in a variety of ways. Still other philosophers, finally, have held sameness and change to be incompatible without supposing either to be a necessary feature of things in the full or true sense. Thus Hume, for example, argued that the attribution of identity to things whose existence is not "invariable and uninterrupted" is a mistake, albeit a mistake that all of us constantly make, but he seems not to have held that things of this sort are any less things, or exist in any less significant sense, than those whose existence is "invariable and uninterrupted."

Against the view that change and sameness are incompatible, however, we have the testimony of Descartes. In the second *Meditation* Descartes tells of taking a piece of wax fresh from the hive and bringing it near a fire. The wax changed, in some respects radically. Yet, asks Descartes, "does not the same wax remain (after this change)? It must be granted that it does remain; no one denies it, no one supposes otherwise." Descartes at least thought that things can change and yet remain the same things. He also claimed that everyone thinks this. If we except

the philosophers mentioned above, this latter claim is surely right. We all constantly acknowledge changes in things, yet continue to count them the same things. People, institutions, buildings, stones, all undergo changes, sometimes beneath our very eyes, without ceasing in our eyes to be the same people, institutions, buildings, and stones. Of course, not all the changes that occur have no effect on self-identity: death is the end of a man and not merely his transformation, and there are many changes that constitute the destruction of their subjects or result in their becoming different things. But in a large number of cases a change in a thing in no way prevents it from staying the same thing, and this is something that all of us take for granted.

I want in this paper to defend the view—Descartes' and everyone else's— that a thing can change and yet remain the same thing, against the philosophical claim that sameness and change are incompatible. This claim is made to rest, by those who press it, upon an argument; I want to show that and how this argument is fallacious. In outline the argument is this. To change is to become different. After it has been heated, a piece of wax is different from what it was before. And if the wax is different it follows that it is not the same: "different" *means* "not the same." Hence, to say that the wax is the same is self-contradictory; to change and yet remain the same thing is, as Parmenides expressed it, "both to be and not be the same and not the same."

The essential steps in this argument are two: it is claimed (1) that "x has changed" entails "x is not the same," and (2) that the "x is not the same" which is entailed by "x has changed" is the denial of the "x is the same" which is used to express self-identity, such that the compound statement "x is not the same (has changed) and is the same (self-identical)" is a contradiction. Now the first of these two steps is unexceptionable; the entailment in question certainly does hold. But the second step is illegitimate. For statements of the form "x is ϕ and is not ϕ" are contradictions only in case the two "ϕ's" (a) are being used in the same sense, or, failing this, (b) are used such that "x is ϕ" in one of the two senses of "ϕ" entails "x is ϕ" in the other of the two senses of "ϕ." And it is easy to show that the two "same's" in "Descartes' wax is the same and is not the same" (a) are not the same "same,"

that is, are being used in different senses, and (b) are not related such that "*x* is the same" in one of the senses of "same" entails "*x* is the same" in the other.

That the word "same" has different senses is a philosophical commonplace. Aristotle drew the distinction that is relevant here (among others), that between numerical sameness ("identity") on the one hand and qualitative sameness ("similarity" or likeness") on the other. And it seems clear, once this distinction is grasped, that it is in the sense of "numerically the same" that Descartes' wax is the same, whereas it is in the sense of "qualitatively the same" that the wax is not the same. It is the same wax; it itself is one single thing. It is only in its qualities—odor, taste, color, shape, temperature, size—that the wax, the one wax, is not the same. This conclusion is confirmed, moreover, when we remember the distinction also drawn by Aristotle between the senses of "change." In one sense of "change," it is the substance, the thing itself, that either comes into being or ceases to be: this is "generation" or "destruction." In the other senses of "change," however, what comes into being or ceases to be is only one or the other of some pair of contrary predicates which may be ascribed to a substance or thing: this is the case with "growth" and "diminution," with "alteration," and with "locomotion." The distinction between the senses of "change" exactly parallels that between the senses of "same," and indeed the two distinctions are explicitly linked in a famous passage in the *Categories:* "The most distinctive mark of substance appears to be that, while remaining numerically one and the same, it is capable of admitting contrary qualities." For one thing to "admit contrary qualities," we learn a few lines later, is for that thing to change in quality, and it is just this that Descartes' wax does.

Hence it is wrong to claim that statements such as "Descartes' wax is the same and not the same" are contradictions on the ground that their two "same's" have the same sense, or are instances of the same "same." But to this the following objection may be made. The distinction between numerical and qualitative sameness (it may be said) presupposes a particular metaphysic, a "philosophy of substance," in which things are regarded as consisting of an underlying substance on the one hand and a set of

qualities or attributes on the other. Both Aristotle and Descartes adopt such a metaphysic; both draw a sharp and clear distinction between the substance of a thing, or the thing itself, and its various attributes or "modes," and it is only on the basis of this distinction that they are then able to distinguish numerical from qualitative sameness: a thing is numerically the same when its substance is unaltered, qualitatively the same when its qualities remain unchanged. But part of the aim of those philosophers who hold sameness and change to be incompatible is just to repudiate any such metaphysic. Hence to appeal to this metaphysic in criticizing these philosophers—which is what we do, covertly, in appealing to the distinction between numerical and qualitative sameness—is to beg part of the very question which is at issue.

This objection can be met. For even if the distinction between numerical and qualitative sameness does rest upon a particular metaphysic, a distinction closely paralleling it can be shown to figure in our everyday, nonmetaphysical thought about the world. Descartes' own statement about his wax may have been couched in the terms of his special metaphysic, but we can construe the statement as a statement of ordinary language, the terms of which are being used in ordinary, nonmetaphysical ways. And when we do, it is plain that the statement's two "same's" are instances, respectively, of two (of the various) different "same's" which occur in ordinary language, the language in which our everyday thought about the world is expressed. Hence it cannot be claimed that the statement "Descartes' wax is the same and not the same" is a contradiction when it is construed as a statement of ordinary language.

Of course, objections may be made in turn to ordinary language, to our ordinary thought about the world. This thought, it may be said, itself embodies a substance metaphysic. But if so, then the original case is conceded, and one may wonder in what thought or language the alleged incompatibility of sameness and change does occur. On the other hand it may be said that ordinary ways of speaking about things and change prove inconsistent upon examination. But the joint attribution to things of change and self-identity cannot be counted as one case of such inconsistency without again begging part of the question that is being examined.

In short, although the claim that ordinary language is somehow wrong to allow a distinction between two senses of "same" is possible, this claim must be defended in some other way than by appealing to the (alleged) incompatibility of sameness and change. And it is only with this alleged incompatibility that I am here concerned.

I have now to establish that two different "same's," two senses of "same," do occur in ordinary (nonmetaphysical) language, and that the two "same's" in such statements as that about Descartes' wax, taken as nonmetaphysical statements, are instances of these different "same's," respectively.

It is common nowadays to distinguish two tasks which the users of language perform, two things they use words to do; and, correspondingly, two functions of language, two uses to which words may be put, or even two kinds of linguistic expression. These are the tasks (or acts) of referring and describing, the referential and descriptive functions or uses of words, and expressions of the referring and describing kinds, respectively. To state a fact of one certain sort, for example, requires, on the one hand, that some definite object or objects be singled out, that it be indicated which thing or things the statement is about (this is the referring task); and it requires, on the other hand, that something be said about what has thus been singled out, that the indicated thing or things be characterized in some way (this is the describing task). When I say, "My car is black," I use the phrase "my car" to indicate what it is I am talking about, hence to refer; and I use the phrase "is black" to express what it is I am saying about my car, hence to describe. "My car" exemplifies the referential use of language, "is black" the descriptive use.

The word "same" occurs in or in association with expressions of both of these sorts, those that are used to refer and those that are used to describe. If I bought the car I now own in 1951, then I can state a fact by saying, "I own the same car now that I owned in 1951." If, furthermore, the car I now own is sufficiently like the one I owned before 1951, I can also state a fact by saying, "My present car is the same as the one I owned before 1951." In the first example, the "same" forms part of a referring expression; the phrase "the same car" indicates or helps to indicate

what I am talking about in saying what I do. (Indeed in this example there is no expression which is used descriptively; the fact it states is not one whose statement requires that anything be described.) In the second example, on the other hand, the "same" is part of a describing expression. In it I use the phrase "is the same as the one I owned before 1951" to say something about my present car, to characterize it; hence this phrase is functioning descriptively.

But not only does the word "same" occur in expressions of different sorts in these two examples, expressions used to perform different linguistic or cognitive acts. The word "same" itself has different functions in these examples; it itself is used to accomplish different things. By using the word "same" in the statement "I own the same car now that I owned in 1951" I *identify* the referents of two different referring expressions, namely, "the car (I own) now" and "(the car) that I owned in 1951." By using the word "same" in the statement "My present car is the same as the one I owned before 1951," however, I *compare* the referents of different referring expressions, namely, "my present car" and "the one I owned before 1951." In the one case I use "same" to indicate that one single thing is being referred to, that the different referring expressions I employ have a common referent. In the other case I use "same" to convey a certain feature of the two different things I refer to: that they resemble one another, are very much or just alike, have some property in common, exemplify a single form or type, or fit a common mold or pattern. (The word "same" can also be used to convey this sort of thing about two different aspects of one and the same thing: I could use it, for example, to compare the present condition of the car I own now with its condition when I bought it in 1951.) What my use of "same" accomplishes in these two cases differs in some of the ways in which referring and describing themselves differ from one another; indeed we may say that identifying and comparing are species of referring and describing, respectively. Thus, comparing x and y is a way of saying something about them; I characterize them by calling them the same in the comparing sense. But I do not, in identifying x and y, say anything about them, or about the single thing to which I doubly refer, any more than I do in just

referring to it, or them, as "*x*" and "*y*." Of course, I must know something about *x* and *y* in order to identify them, just as I must in order to refer to them, or it, as "*x*" and "*y*." But this knowledge is not what I express when I say they are the same in the identifying sense; in calling them the same in this sense I do not *say* what it is about them that I know and that makes me or enables me to call them the same: at the most I *imply* it in some sense or other, as Strawson has put it. In any case identifying is not, like comparing, a form of describing, and identificational sameness is not a descriptive concept.

In the examples so far discussed, the word "same" has actually been part of compound expressions used to refer on the one hand and used to describe on the other. But not all the "same's" that occur in our language occur as parts of such expressions in this way. Nor does our recognizing a given "same" as a "same" of identification or of comparison depend upon our finding it to be a part of some larger expression which we recognize as a referring or a describing expression, as the case may be. If it did we should not be able to distinguish the "same's" in such statements as "The morning star and the evening star are the same" on the one hand, and "The dress my wife is wearing and the dress my neighbor's wife is wearing are the same" on the other; whereas in fact we have no trouble either in distinguishing them or in recognizing them as "same's" of identification and of comparison, respectively. It seems that we are able to tell when words are being used for the specific purposes of identifying and comparing quite as readily as we can tell when they are being used for the generic purposes of referring and describing, and that this ability is what at bottom enables us to recognize, in particular cases, the sense in which the word "same" is being employed.

The fact that "same" is used to accomplish different things, to identify on the one hand and to compare on the other, is sufficient to justify my claim that the word has different senses in ordinary language. But there are some further differences between the "same's" of identification and comparison that it may be useful to point out. First, the "same" of identification cannot be qualified; it applies absolutely or not at all. A comparative "same," on the other hand, is always qualifiable. It makes no

sense to say that the morning and the evening stars are more or less the same (identifying sense), nor, accordingly, that they are just the same, or perfectly the same. Either they are the same or they are not, since they are either one thing or two: no in-between is possible. But the dresses worn by my wife and by my neighbor's wife, respectively, may be more or less the same (comparing sense). Indeed if they are not almost or nearly or practically the same, or any less the same than this, they must be just or exactly or "identically" the same. Comparative sameness, we may say, is relative; it admits of degrees, including the highest; whereas identificational sameness is an absolute, all-or-none concept.

A second difference between the two "same's" concerns the way they are applied to things, the basis upon which things are said to be the same in the different senses of the word. I may simply see that two things are the same in the comparing sense, and when I say they are the same in this sense I do no more than report what I see. The sameness of the dresses worn by my wife and by my neighbor's wife is a natural feature of them, one that I may discover just by looking at the two dresses. But I never simply see that two things are the same in the identifying sense. At the most what I see is that they satisfy certain *criteria* of identificational sameness, but when I call them the same in the identifying sense I say more than this: I express a *conclusion* I have drawn from the fact that the criteria are satisfied. The sameness of the morning and the evening stars is not something I just discover, by looking or by any other means; it is not a natural feature of the two stars or of the one planet Venus. It is rather something I *infer*, and something I ascribe as much by following *conventions* as by discovering *natural* features. The conventions connect what I see with what I infer; they are rules which specify the criteria of identificational sameness and, hence, which warrant or provide for the step from what I see or otherwise discover about the two stars to the conclusion that they are one and the same thing, namely, the planet Venus. Ascriptions of identificational sameness always involve the use of such conventions, whereas ascriptions of comparative sameness often do not.

The difference here is brought out by the following consideration. If someone says that two things are the same in the compar-

ing sense, having seen that they were, and later learns that the two things are different in ways he did not see, he will not in general *withdraw* his ascription of sameness but will rather *qualify* it, saying, for example, that the two things are the same in all or certain observable respects. But if someone says that two things are the same in the identifying sense on the basis of what he has seen of them, then the discovery of new information may well lead him to withdraw his ascription of sameness altogether: "I thought they were the same, but I realize now that they are not." This last situation is of course possible not only because the ascription of identificational sameness involves the use of criteria, but also because there are *various* criteria of identificational sameness, and sometimes these conflict with one another. That is, there are cases in which some criteria of sameness are satisfied and others are not. In some such cases we are able to decide the question of sameness by deciding, as it were, which criteria shall take precedence, but in others we are unable to say whether two things are the same or not. The fact that cases of either of these sorts are possible has the important consequence that "x and y satisfy some criterion of sameness" does not entail "x and y are the same." This means that the step from seeing or otherwise discovering that the morning and the evening stars, for example, have certain natural features, to the conclusion that they are one and the same thing, is not a deductive step. Of course it does not follow that it is an inductive step, or that what is seen in such a case functions as empirical evidence for the conclusion that is drawn: the connection between criteria and that of which they are criteria is not a contingent one.

Given the conventions of our ordinary thought and language, comparative sameness is itself a criterion of identificational sameness—perhaps, after spatio-temporal continuity (which we also discover in many cases just by looking), the most important one. This fact alone shows that the two concepts are different, but it also shows more than this: it shows that they are concepts of different *kinds*.

I have now to establish that the two senses of "same" that I have been examining are the ones exemplified, respectively, by the two "same's" of our specimen statement about Descartes'

wax. To do this it is only necessary to call attention to what a speaker in the circumstances described by Descartes in the second *Meditation* would want to say, to the information he would want his statement to convey. He would want, on the one hand, to note that the wax had changed, and this would require him to compare the condition of the wax after heating with its condition before heating. To say that it is not the same, in the comparing sense of "same," would be one natural way of doing this. On the other hand, a speaker in the circumstances described would want to indicate that one single thing was being referred to, before and after, notwithstanding the difference of condition or state; otherwise it would make no sense to say even that the wax had changed. He would want, in other words, to identify the referents of such referring expressions as "the solid piece of wax that Descartes held before the heating" and "the puddle of clear liquid that he held after the heating." Here again the natural way of doing this would be to say that the same wax remained after the heating, or that the earlier piece and the later puddle are one and the same thing, the same wax, "same" this time having the identifying sense. The compound statement, "Descartes' wax is both the same and not the same," would be one way of conveying both these pieces of information together, and there is no doubt that the two "same's" in it would have the functions, respectively, of identifying and comparing.

It seems clear, therefore, that the claim that such statements are contradictions is incorrect on the assumption that its two "same's" are being used in the same sense. But what of the other possible basis for this claim, the assumption that the two "same's" are connected by a relation of logical entailment? This assumption, too, can be shown to be false, at least when the statement is construed, as we have been construing it, as a statement of ordinary language. For the assumption amounts to this, that cases in which an x and a y are the same in one of the two senses of "same" and not the same in the other are logically impossible. But in fact such cases do occur, which is to say are not excluded by the rules whereby words are used in ordinary language. The car that I owned in 1951 and the car that I own now are one and the same car, but since its engine has lost its power, its fenders have been dented,

and it has been painted a different color since 1951, the same car is different now, is not the same as it was in 1951. The fact that I can correctly identify the referents of "the car that I owned in 1951" and "the car that I own now," that I can truly say that they are the same in the identifying sense, does not at all rule out the possibility of their being not the same in the comparing sense, the possibility that in order to describe my car correctly I should have to use different, even contrary descriptive phrases as predicates of the two expressions whereby I refer to it, when these are used as subjects. And this means that comparative sameness is not entailed by identificational sameness. Actually, it is only this entailment which would justify the claim that statements such as that about Descartes' wax are contradictions, but the converse entailment can be shown not to hold as well. For suppose that the car I own now and owned in 1951 is exactly like the car I owned before 1951; suppose, that is, that they are exactly the same in the comparing sense, that any descriptive phrase which could be used correctly to describe the one could also be used correctly to describe the other. It is still perfectly possible to suppose that the car I owned before 1951 and the one I owned in 1951 and have owned since are different cars. Comparative sameness, accordingly, in even the highest degree, does not entail identificational sameness.

This last point shows the falsity of the Identity of Indiscernibles principle; and I think the mistaken assumption that identity—my identificational sameness—is entailed by indiscernibility—my comparative sameness—is what has led philosophers to adopt this principle. This assumption seems to rest in turn upon a failure to understand the true nature of the relationship between the two sorts of sameness. The latter, I argued, is a criterion of the former, but only one among a number of criteria. Hence, although the relationship is logical and not empirical, it cannot be one of entailment. But the distinction here is fine, and this gives some excuse for thinking that the Identity of Indiscernibles principle is true. The same cannot be said, however, for the related view that I have been examining here, the view that sameness and change are incompatible. It is hard to believe that philosophers who have held this view have simply confused the two sorts of sameness

that I have been concerned to distinguish. But then they must have supposed that identificational sameness entails comparative sameness, and for this supposition there is not the excuse that the actual relationship between the two concepts is similar to that of entailment in certain respects. For identificational sameness is not a criterion of comparative sameness, as the latter is a criterion of the former. Indeed there are often no *criteria* of comparative sameness at all, if what I have said about the way this concept is ascribed to things is true.

One final word. I said earlier that the distinction between identificational and comparative sameness, which is a distinction between concepts that figure in our ordinary thought about the world, closely parallels the metaphysical distinction between numerical and qualitative sameness. I think that the latter is in fact intended as an explication of the former, and that the one simply is the other divested of its metaphysical trappings. Hence I would have no objection to saying that the distinction between numerical and qualitative sameness itself occurs in ordinary (nonmetaphysical) thought and language. But even so, I think there are advantages to describing the distinction in the functional terms that I have been employing here. For talk of numerical and qualitative sameness is liable to encourage the view that these are different concepts of the same general sort, that numerical and qualitative sameness are two species of a single genus; whereas I have tried to show that the use of these two concepts (or their functionally described counterparts) accomplishes different things, and hence that the two concepts are not merely different but are concepts of different kinds. The particular danger is that numerical or identificational sameness will be thought a descriptive concept. Sameness is something that we attribute or ascribe to things, but it is only when the sameness we ascribe is comparative or qualitative that, in doing so, we describe anything. This fact is brought out when the two concepts of sameness are described in terms of their function or use in language and thought, but not when they are merely referred to as concepts of sameness, as it were of the numerical and qualitative *varieties*.

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DISCUSSION

THE MEETING OF EXTREMES IN CONTEMPORARY PHILOSOPHY*

If Dr. Mure's *Retreat from Truth* had an epigraph, it would certainly be that dictum from *Appearance and Reality* to which he obliquely refers in his preface: "Where all is rotten it is a man's work to cry stinking fish." That all is rotten, that "spiritual sterility" is the leading characteristic both of contemporary poetry and of contemporary philosophy, he does not for a moment doubt; and if he cries "stinking fish" in a somewhat muted tone of voice, without the Billingsgate vitality the phrase might suggest, his despair is nonetheless genuine and bitter. "If at present," he concludes his jeremiad, "I had an intelligent son coming up to Oxford, I should not regret it if he turned his face away from all the three honour schools that include philosophy, even from Greats" (p. 250).

The contrast in mood with Professor Morton White's *Toward Reunion in Philosophy* could scarcely be more striking. White is by no means complacent about the twentieth-century philosophical scene; he subjects Moore, Russell, and Carnap to lengthy, close, and critical examination. But the general tone of this criticism is optimistic, ebullient, sometimes a little tiringly so. White writes as one of the family, whereas Mure is self-consciously an outsider. The reunion White envisages would be a new alliance between positivism and pragmatism; these are already united in Mure's mind as different sides of the bad empiricist penny. White's attitude to Mure's ecumenical maneuvers would be very like a Protestant evangelical's attitude to proposals for the reunion of the Anglo-Catholic and Roman Catholic Churches: Tweedledum at last recognizes his identity with Tweedledee. The only reunion which would interest Mure is a convergence and revitalizing of the scattered forces of idealism, and of that he sees no real prospect, in England at least.

Oddly enough, however—and for all this difference in manner, tone, and concern—White and Mure arrive at conclusions which at least run parallel. White would not care for Mure's phrase, "the unity of value

* G. R. G. Mure, *Retreat from Truth*. Oxford, Basil Blackwell, Ltd., 1958. Pp. 250. 31/6. Morton White, *Toward Reunion in Philosophy*. Cambridge, Mass., Harvard University Press, 1956. Pp. 299. $5.75.

with reality and truth"; Mure might think that White's proposals for a "federal union" of ethics, science, and logic are expressed too much in terms of forms of inquiry, are too little ontological, but both are attacking the sharp contrast between "questions of fact" and "questions of value" which has characterized so much of the thinking of our time. Nor is this—on both sides quite unenvisaged and unwelcome—"meeting of extremes in contemporary philosophy" as surprising as it at first sight appears to be. Many years ago, in his *The Origin of Dewey's Instrumentalism*, White emphasized the historical affiliations between absolute idealism and pragmatism; his own position, if he is a little reluctant to call it pragmatism, at least brings him so close to Dewey that the spirit of Hegel can be heard breathing heavily in the background.

The central point in the philosophical revolution proclaimed by Moore and Russell in the early years of the present century is its emphasis on objectivity. For Moore and Russell the inquirer is essentially a discoverer—whether what he discovers are empirical facts or conjunctions of meanings. Truth, as we might put the matter, is there to be found—hidden, perhaps, at the bottom of a well but still "out there." And so are meanings. Between—to use Freudian language—the "reality principle" and the "pleasure principle" there can be no co-operation and ought to be no compromise; what a philosopher would like to be the case, what he supports, prefers, or decides in favor of, is a matter for him, merely, as a private citizen. As a philosopher, only one question concerns him: *what is actually the case?*

It would be putting the matter too strongly to say that for the idealist and the pragmatist, on the contrary, truth is of our making, meaning lies in our free gift, and the reality we profess to find, in science at least, is a purely human construction. Bradley and James, especially, have too much respect for the objective spirit of science to adopt quite this position. But it is interesting to find that Mure criticizes Bradley precisely for not doing so; Bradley (and Bosanquet), he writes, "seem to me to retain too much of the empiricism they set out to counter, and unduly to assimilate philosophical to scientific thinking" (p. 243). White, similarly, criticizes Peirce and James on the ground that they did not wholly appreciate the anti-objectivist tenor of their own teachings (p. 269). Both Mure and White are in search of what White calls a "philosophy as varied and full as life itself" (p. 249), and they both hope to achieve this end by breaking down barriers between finding and making, discovering and evaluating.

The type of idealism espoused by Mure will no doubt surprise Anglo-Saxon readers, nurtured as they are on the idea that the typical Oxford idealist was Bradley—as if Bradley could be typical of anything or of anywhere! It is easy to forget, because he wrote so little, that J. A. Smith, admirer and interpreter of Croce and Gentile, held the Chair of Moral and Metaphysical Philosophy from 1910 to 1935, and equally easy not to realize, because he does not sufficiently emphasize the fact himself, that the thinking of Collingwood, Smith's successor in the same Chair, largely derives from those same sources. If there is a "typical idealism" in twentieth-century Oxford, it is, indeed, Italian idealism. For the Italian idealists, as Mure puts the matter in his preface, "science is an essentially practical matter and empiricism is an *ersatz* philosophy, which falsely identifies in kind philosophical and scientific thinking, and therefore cannot envisage any connection between reason and intrinsic values" (p. vii). Such an essentially technological philosophy, Mure agrees with its Continental critics, flourishes quite naturally in a practically-minded country like England. In the later nineteenth century, so Mure suggests, British philosophy temporarily broke through the bonds of practicality, but this brief incursion into spirituality did not last. "Idealism was a borrowed philosophy, and when we became engulfed in the miseries of hot and cold warfare, and the expanding physical sciences began to take an ever-increasing toll of the intelligentsia, we rapidly forgot it and returned to our Humian vomit" (pp. 72-73). The characteristic feature, for Mure, of the empiricism thus inelegantly described is that for it "there is no world but the world as the practical agent envisages it in order to act" (p. 20).

I should not myself wish to deny that the traditional empiricist sometimes takes a crudely economic view of human thinking and, more generally, of human activity, as if the only human concern were profit and the world were no more than a giant market place. My complaint is rather that Mure has been infected by the same practical approach; in particular, he has simply taken over that "instrumentalist" conception of science which is the most striking feature of the vulgar sort of empiricism. "Save when knowledge is power," wrote Mure, "why should any man desire passionately to study that in which nothing is intrinsically better or worse than anything else? . . . To explore such a material world, except in order to exploit it where one can and evade it where it threatens irresistibly, would appear to be a quite irrational waste of time" (p. 35). Thus, for Mure, the scientist who seeks knowledge of the natural world just because he wants to know how things

happen is simply wasting his time; science is either a means of exploiting the world or of evading its dangers—precisely the view of the crudest sort of Philistine, for whom "science" consists in technology and medical research.

Berkeley was no doubt right in arguing that if our knowledge of material objects is reducible to an acquaintance with transitory sensations, science can indeed be no more than this—an aid to the conduct of life, based on that sort of practical sharpness Plato allowed to the prisoners in his Cave. That view of the matter, of course, is wholly dependent on the traditional empiricist analysis of perception. Mure seems to want to rest his case, rather, on a direct intuition that scientific inquiry is valueless. The fact remains that his conception of science is precisely that of the "economic man" he generally despises. And this conception of science, as he says, is crucial; "the whole position for which I contend" rests on a denigration of science. For he wants to argue that if we take science seriously as a form of thinking—if we think of scientific thinking, as the empiricist does, as the paradigm case—we are bound to fall into a practically-minded sterility.

How is one to reply to such a charge? "The starry heavens," writes Mure, "save to the aesthetic eye have grown duller and duller, progressively more pointless. The effort to sink parochial pride and explore without prejudice—if that is what science is—has produced no glimpse of a metropolis" (p. 35). A monotonously twinkling array of "mere objects"—something we could contemplate but never understand—has been converted for us by modern astronomy into vast new worlds of motion and change; a field of inquiry has been opened up to us, the study of stellar processes, which excites the utmost resources of the human imagination; I just do not know what to say to anybody who thinks that as a result the heavens "have grown duller and duller." Certainly the stars do not form a "metropolis," but why that should be thought to be a disadvantage, I cannot for the life of me imagine.

For Mure "no truth can have value for its own sake except it be the truth about that which has value for its own sake." But why? Let us put the matter in terms not of "truths" but of inquiry; for it is the economist who values ends, while the ethical theorist concerns himself with the value of forms of human activity. There is no good or bad *per se*," writes Mure, "except the Mind's self-conscious activity" (p. 178). One can see that a person might for various reasons believe that ethical inquiry is more useful than physical inquiry. But on what possible grounds could it be claimed that it has greater intrinsic value?

For inquiry into the valuable has the same general characteristics, as a form of human activity, as inquiry into the nonvaluable. What Mure says of aesthetic activity—that it is "a partial union of man with an activity which he feels as his but as also beyond him, a co-operation or a conspiracy which can be called 'creative' not because it is practical but because human nature moves on in the phases of it" (p. 181)—is surely equally true of the activities of the scientific investigator. The whole language of "self-transcendence" makes better sense as a description of scientific inquiry than it does in respect to any other field of human activity.

Mure's argument on this matter exhibits the sort of ambiguity and confusion, the logical looseness, of which one had hoped philosophy had seen the last. "The only intrinsic value or good *per se*," he writes, "is a mind's self-conscious activity. Hence value and consciousness of value are in a sense identical." Consider this "hence" and "in a sense." From the fact, if it is a fact, that nothing is valuable except self-conscious activity, it does not even follow that it must be conscious of its own value. Granted, even, that an artist has to be self-consciously an artist if his artistic activity is to be valuable—a startling enough supposition—it still will not follow that he must also be conscious that being an artist is valuable. (What vistas of priggishness this opens up!) Nor, even if he must be so conscious, does it follow—even "in a sense"—that the value of the activity and his consciousness of its value are identical. Suppose, to take another example, it were true that a man could not be courageous without knowing that he is courageous and knowing that his courage has value, it still would not follow that his knowing he was courageous is identical with his being courageous. Indeed, if this identity did hold, then the apparently informative statement "to act in a valuable way one must be conscious that one is acting in a valuable way" would collapse into a mere tautology. Idealism, that is, is here up to its old trick of parading statements which if naturally interpreted are false but informative and if idealistically interpreted are tautologies, in the hope that we shall be induced to suppose that they are both true and informative.

I fear that Mure is right in his admission that "there is plenty to be said against the rough-and-ready exposition of idealism and transcendence with which I have filled the last few chapters. It will appear both unoriginal and in itself doubtfully consistent" (p. 243). What is really disheartening is that although he emphasizes "the need for the critic to read with sympathy and with historical imagination before he passes to the criticism which is the purpose of his reading" (p. 248), he

seldom shows any sign that this is the spirit in which he has read the critics of idealism.

Not that his polemics against traditional empiricism are wholly worthless; it is easy to understand why absolute idealists, confronted by Russell's logical atomism, should have dismissed it as the blackest reaction. The sort of criticism of the "simple given," the "object of acquaintance," which Mure sketches on pages 50-51 and T. H. Green had more thoroughly developed, was by the revivers of empiricism simply and quite unjustifiably ignored. (This becomes less puzzling historically when we remember that the philosophy which Russell and Moore at first set against absolute idealism was not, in the traditional sense, empiricist and, as well, that they were Cambridge men.)

My complaint against Mure is that he does not push this line of criticism hard enough; he uses it merely to depreciate science. For science, the suggestion is, the conception of absolute "fixed points," as White calls them, in the form of simple given experiences is wholly appropriate. But surely it is as inadequate as an account of science as it is of any other form of inquiry; if, as Mure puts it, "any allegedly simple datum will turn out on examination not only to possess internal diversity but also to be the product of analysis and selection" (p. 50), then the traditional empiricist account of science is wholly untenable. Mure is content to say, rather—in the manner characteristic of absolute idealism—that empiricism "operates at a low level." This leaves him free to develop his conception of the philosophical as a "higher level" of thinking; whereas to reject outright the traditional empiricist conception of science is at the same time to destroy this supposed gradation of levels, not by abasing philosophy but by elevating science, permitting us to see in them equal collaborators in inquiry neither of which possesses absolute sovereignty, whether in a private realm of its own or in the whole territory of human thinking. This, in general terms, is also White's view; but he asks us to pay a price for his conclusion which brings him uncomfortably close, as I have already suggested, to Mure's form of idealism.

For Mure "in philosophical thinking, and perhaps in serious historical thinking"—the proviso marks the admirer of Italian idealism —"finite judgment, so far as it is true, clearly does not copy and correspond to anything; it manifests and actually sustains the objective world which gives it content. Outside the world of economic observation, truth in its universal, non-private aspect is an infinite activity which constitutes itself in constituting finite minds, not a character of independent fact nor a relation of correspondence between independent

fact and finite human minds" (p. 233). I should not care to be called upon to explain exactly what this means, how true finite judgments manage at once to "manifest" and "sustain" an objective world, or how "truth" can be an activity, and more particularly an "infinite activity" which "constitutes itself in constituting finite minds." Yet it would be a mistake to cry "Nonsense!" and let loose the dogs of positivism. However obscurely (and in the idealist tradition of "having it both ways") Mure is here confronting what are genuine philosophical problems. We might well refuse, as he refuses, to describe truth as a character of independent fact—to maintain that some facts are true just as some roses are red—and hesitate to define it as correspondence to fact. (For what does "correspondence" mean? Will anything short of identity do the trick? And to what fact does the assertion correspond that "*p* corresponds to *f*"? To the fact that "*p*" is true if and only if "*f*"? Then don't we need "truth" to define correspondence?) And then, in reaction, we might well be tempted to say that "truths" are something we make, and yet at the same time that they are not of our *individual* making—this is why Mure introduces the infinite mind, so as not to lose all hold on objectivity—and even that they make us, as well as our making them, in so far as they form the content of our mind. Mure may garb these temptations in singularly untempting clothes, but not so as wholly to conceal their fascinations.

Certainly White is conscious of these temptations, or some of them. "Those who imagine that we first find out whether something is true," he writes, "and then decide whether we ought to believe it think of truth as correspondence to ready-made Platonic facts; they adopt a spectator's view of the matter, rather than a view of the man who is doing science, i.e., making truth and falsehoods in the sense of deciding what sentences should be assigned what truth-values, i.e., accepted as true" (p. 271). To both Mure and White, that is, there is a similar opposition between the idea of truth as something to be found (it being presumed that "finding" would be the discovery of correspondences) and the idea of truth as something to be made. How is White, working in the general tradition of recent philosophy, led to such Mure-like conclusions?

In a Hegelian manner, he begins by dividing recent philosophy into three periods, which he calls respectively the age of meanings, the age of words, and the age of decision, of which last age he is prophet as well as representative. "The age of meaning" he might less misleadingly have called "the age of existence" or "the age of the objective issue." In this "age"—most fully depicted in the early writings of Moore and

Russell—it is presumed that philosophical questions are to be answered by referring to the fact that so-and-so exists. If then we ask; "What does the predicate '*x*' mean?" a good answer will assert the existence of some attribute to which '*x*' refers; if we ask, "What is the difference between analytic and synthetic propositions?" a good answer will point to distinguishing properties of analytic propositions, for example, to the fact that their predicate is contained in their subject; if we ask, "Are there physical objects?" a good answer will either be a demonstration that there are such objects (Moore's) or evidence in support of the hypothesis that there are (Russell's). The goodness of these answers White wholly denies, in a long series of vigorous and effective, if sometimes too rapid, critical examinations of Russell and Moore.

In his dissatisfaction with the ontological approach to philosophy, White is at one with the leading representatives of the "age of words." For them questions about "meaning" are requests for information about the part played by an expression in a given linguistic system; a meaningful expression is one which has a prescribed use in such a system. Analytic propositions, for the "informal" wing of the linguistic movement, assert the synonymity of two expressions in some natural language; for the formalizing wing, the analyticity of a proposition consists in its following immediately from the rules of the language. As for questions of existence in general, these are either dismissed as nonsense or settled by arbitrary stipulation; the philosopher admits or does not admit certain kinds of expression into his system, allows or does not allow certain methods of formulating statements.

White's general objection to this method of approach—once more he makes a large number of detailed and interesting criticisms—is that it converts philosophy into a wholly arbitrary procedure. This comes out most clearly in his criticisms of Carnap. (Except on special points White in fact says very little about "ordinary language" philosophy.) Carnap's approach amounts to saying that "we simply call" certain expressions analytic or meaningful. Then the natural question is, "But why should we distinguish certain sentences and expressions in this way?" It is easy enough to see the point of the religious ceremony of baptism, but what is the point of saying to a sentence, "I name thee 'analytic'?" Baptism frees a child from original sin, or makes him a member of a church, or gives him a (socially useful) proper name. But what corresponds to this religious or social purpose in the case of "meaningful" or "analytic"?

White has now arrived at a question which contains the word "should"—why *should* we single out certain sentences in a special way?

—and this enables him to identify what have traditionally been thought of as problems of logic and epistemology with what are now commonly described as "ethical" problems, that is, problems of decision making. Logic, ethics, and epistemology—so he is arguing—all set out from problems of the same form: under what conditions are we justified in coming to such-and-such a decision?

His position is perhaps at its most plausible in regard to definition, a procedure to which he pays a great deal of attention. It has sometimes been supposed that there are formal properties which determine that, say, *BC* rather then *DE* is a definition of *A*. But although such properties may be necessary if *BC* is to be a good definition of *A*, they are not sufficient; we cannot explain in purely formal terms why we define man as a rational animal rather than as a featherless biped. To fix upon a definition is to take a decision which we can be asked to justify. We might justify the definition of man as a rational animal rather than as a featherless biped, for example, by asserting that rationality is a "more important" property of man than "featherlessness," but it is quite apparent that estimates of importance cannot be derived by mere contemplation of the world or of formal relations. So, White concludes, "I have converted the apparently descriptive question of . . . definability into a quasi-ethical question which is not reducible to a metaphysical question about the properties or relation of attributes 'out there' " (p. 195).

There are two problems which we shall need, as White does not, carefully to distinguish from one another: the first, whether questions of definability can be converted into questions about the relations between attributes, and the second, whether, if this is so, questions of definability are essentially metaphysical in character. Now, in White's thinking, these two problems are really identified; that is, he presumes that as soon as we talk about "attributes" we are committed to what he calls "Platonism," against which the main force of his polemics is directed. But is this really so? Suppose, by asserting that things have characters or attributes, we simply mean that they go on or behave in regular ways or display certain patterns; then these "regular ways" or patterns are not entities. To say that "James has courageous attributes" is not to relate James to an entity *courage*; nor does "the attribute of courage is bound up with the attribute of intellectuality" relate entities one to another. It asserts, simply, that anyone exhibiting courageous behavior will also be exhibiting intelligent behavior; and, similarly, "fire is hot" asserts that anything behaving in a fiery manner behaves hotly. Or this is at least a possible view; in other words, the

choice does not lie, as White seems to think, between Platonism and some variety of nominalism.

One can unhesitatingly agree with White, similarly, that understanding, evaluating, defining, prescribing are activities we carry on, and that they do not depend for their possibility on the existence of special, odd types of entity—meanings, attributes, values, real natures —without at all concluding, as he does, that they are "modes of *linguistic* behavior." Suppose a wine-taster is at work. He picks up a glass of wine, sniffs it, rolls it around in his mouth, and looks up at the heavens reflectively. Isn't he evaluating? Would it make any essential difference to the situation if he now proceeds to speak or write something down? Suppose, too, he does write something down. How do we know that what he writes down is evaluative? One possible answer, of course, is that we know it is evaluative because it contains such-and-such words, as White specifically asserts in the case of prescription. "We prescribe a course of action," writes White, "when we affirm a sentence which contains the name of that action followed by the phrase 'is right' or 'ought to be done.'" But this will not do at all. I cannot think of a single sentence containing the name of an action followed by the phrase "ought to be done." (Giving a present to your wife "ought to be done"—surely this is not how we prescribe!) More importantly, a commissionaire might say, "Boxing is right; wrestling is left," without prescribing. The same applies to evaluation. There are no doubt certain words which we ordinarily use when we are evaluating. But how do we judge that a word is being used evaluatively? A stranger comes to Australia and hears somebody described as a "drongo." He has never heard the word before; but he knows that it is evaluative because it arises within a certain form of social behavior—in a situation which "calls for" evaluation. In other words, he recognizes that a word is evaluative by seeing that it is used in an evaluating situation, not vice versa.

It is best to concentrate attention, in philosophical discussion, upon specific evaluative expressions like "drongo" or (of wines) "robust," because the more general evaluative expressions like "good" and "bad" are so context-free as scarcely to be descriptive. We may easily be led to assert that "good" refers to a nonnatural predicate, or a "value," or alternatively that it only "expresses an attitude," unless we concentrate upon such special contexts as good dinner, good violinist, and the like. Then we should see—what is apparent in the case of a word like "drongo"—that to call something "good" is to raise an issue of fact, even though the issue will vary with the context.

White would object to this way of putting the matter, just as he objects to what one might also wish to say—that to evaluate is to characterize—on the ground that to talk of facts is "Platonic"; when he has to use the word "fact" he puts it in what Ryle calls "shudder-quotes"; he refers to facts as "hypostatisations." This, no doubt, is because he associates facts with the correspondence theory of truth; or with the conception of a world that is "simply there," a world given to us. But these last two ways of putting the matter ought not to be, as they tend to be, identified. It is surely absurd to deny that the atmosphere on Venus is either "simply there" or is "simply not there." No doubt it is we, as human inquirers, who select Venus's atmosphere (rather than, say, that atmosphere and the space surrounding it for a hundred miles or so) as a subject for inquiry. "The atmosphere" isn't simply given to us as a specific object; we "make the object" by selecting certain boundaries. And the same is true of "Venus." Furthermore, it is only by effort and struggle that we can find out whether Venus is characterized by the presence on its surface of an atmosphere. In all these important respects the fact that Venus has an atmosphere is not "given."

Yet all the same there being an atmosphere on Venus is not something we *decide*, in the sense in which we might decide whether to buy an elm for our garden. Whether the world is to contain "an elm in our garden" depends upon our decision, but whether, once we have put the elm in our garden, it is or is not there does not depend on us; and even more obviously it does not depend on our decision whether certain conditions do or do not exist on Venus, although the selection of these particular conditions for study, and the discovery what other characteristics they possess, is our doing. We do not *find* what happens but rather we *find it out;* facts are not given, but rather they are *taken to be.*[1]

White seems to think that he has to say either that we *make* truths, or else that truths "correspond to facts." He then plumps for the former alternative, as in the passage I have already quoted. But surely there is no sense of "making truths and falsehoods," no circumstance in which somebody might say, "Yesterday I made three truths and five falsehoods," as he might, intelligibly if unidiomatically, assert that "yesterday I made five classifications and three definitions."

No doubt a scientist has sometimes to make the decision whether or not a proposition is true. Even this is a relatively rare case; more

[1] Of course, in ordinary English, the idiom "I take it to be a fact . . ." has a limited use; as has the idiom "given that. . . ." I do not pretend at this point to be describing, or conforming to, ordinary English linguistic customs.

commonly he takes a proposition to be true, for example, that a bottle contains hydrochloric acid, without ever "coming to a decision" that it is, just as, more generally, we ordinarily act in a certain way without at all deciding to do so. But although the scientist might make a remark and then, after it has been criticized, make the decision that it is true, he certainly cannot make it true.

It is not really a surprising conclusion that definitions involve decisions: constructing a decision is a deliberate process; one decides on a definition after hesitation and consideration of alternatives. On the other hand, taking a proposition to be true is not in general a deliberate process. What leads White to think otherwise is his nominalism; whereas we would normally say that we find out, or notice, or observe, or discover that something has a certain characteristic—and, only rarely, *decide* that it has—the nominalist has to say that we deliberately apply a general term to it, that is, that finding that x is y really consists in deciding to apply the general term y to x. One can see why he comes to this conclusion if he supposes that the only alternative is to say that "we first find out whether something is true and then decide whether we ought to believe it," or that we are "given" a connection between universals, or that we are confronted by certain sense data. But if finding out is not "being given," it is not "making" either.

With much of the polemics of Mure and White I completely sympathize: with Mure's rejection of traditional empiricism, with White's rejection of Platonism. Furthermore, some of the things Mure more particularly says about inquiry seem to me (however obscurely expressed) to be true—as much true of science as of philosophy. As for White, the general conclusion at which he arrives—that all propositions are open to challenge, that inquiry proceeds against a background of unchallenged but not unchallengeable assumptions—is one with which I should be almost wholly in accord. What is really encouraging in contemporary philosophy, I should say, is the appearance there of a genuine empiricism which takes seriously the view that any informative statement is in principle questionable, without being committed to what are essentially Cartesian presuppositions about experience—presuppositions, that is, that really belong to the a priori, rationalistic, mode of thought. (Simple ideas are "clear and distinct perceptions" in disguise.) That "the root epistemological phenomenon is that of firm belief," that to call a proposition a priori is just a way of emphasizing that we are not going to give it up in a hurry, that there are no good grounds for picking out a class of beliefs as "analytic"—these are all

conclusions which we might hold, if we want to, quite independently of White's nominalistic emphasis on decisions.

What I have wished to question is only the doctrine of the omnipresence of the ethical—whether it takes the form of asserting that the only valuable sort of inquiry is inquiry into the valuable or, rather, that the question "Should I decide in such-and-such a way?" lies at the center of every inquiry. Moore and Russell were right, I want to say, in asking "What is the case?," wrong in supposing that an answer to that question had to refer either to "meanings" or to simple sense data.

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MATHEMATICAL IDEALS AND METAPHYSICAL CONCEPTS

In his recent series on "Criticism in the History of Science: Newton on Absolute Space, Time, and Motion"—*Philosophical Review*, LXVIII (1959), 1-29; 203-227—Professor Stephen Toulmin has argued that the concepts of absolute space and time functioned, in Newton's scientific work, as "mathematical ideals"—as formal elements of an axiomatic system—rather than as empirical postulates.

The temper of empiricist philosophy since Mach has been to label such clearly nonempirical concepts as "absolute space" and "absolute time" as "metaphysical," and therefore and therewith to dismiss them as "meaningless." Toulmin's account is a suggestive addition to the growing number of attempts to rectify this unbalanced view of the logic of science. But although I agree with him that such concepts can be interpreted as playing a significant role as elements of a nonempirical side of a theory, I do not feel that he has done justice to this point. The expression "mathematical ideal" (instead of which, for reasons he does not explain, he often uses "dynamical ideal") not only is left too vague to explain the role of the concepts in question, but even is so misleading as to falsify that role and to produce important distortions in Toulmin's own account. Once we have clarified that role, it will appear that the employment of these concepts, even in this nonempirical yet nonmetaphysical way, need not be, and was not in classical physics, as guiltless of violating sound scientific attitude as Toulmin suggests it was.

The only explanation Toulmin offers of his notion of a "mathematical ideal" occurs on pages 16-18; the core of his discussion is contained in the following passage:

> Suppose we make a pendulum clock as accurately as we can, and compare the rate of swing of the pendulum with the motion of the sun across the sky; we shall find that they do not proceed exactly in step, their relative rates varying between the equinoxes and the solstices. What are we to make of this discovery? If we use the "natural" or solar day as our measure of "vulgar time," we shall be obliged to say that the pendulum swings at a rate which varies slightly according to the season of the year, and that this variation persists however much we improve the construction of the pendulum. (This by itself might be a genuine phenomenon calling for a physical explanation.) But now suppose in addition that we carefully observe the motions of Jupiter's satellites, timing the instants at which they are eclipsed once again by solar time: in that case,

we shall find a precisely similar seasonal variation. Yet who will want to concede that the rate of circulation of these satellites around Jupiter is affected by the season of the year—the season, that is, not of the jovial but of the terrestrial year? We clearly do better to regard the pendulum and the circumjovial satellites alike as moving more uniformly than the sun across the sky, hoping in due course to explain this variation in the sun's apparent motion as the more genuine phenomenon. . . . If we accept this alternative, however, that will mean abandoning the previous "vulgar" measure of time and taking in its place for theoretical purposes a dynamical ideal: namely, the common ideal toward which both terrestrial and celestial motions—pendulums and satellites—are found to approximate. In these so nearly uniform motions we see the closest physical realization of the "absolute, true and mathematical time" of Newton's axiomatic system [I, pp. 16-17].

According to this account the reasoning behind the rejection of the solar time scale is as follows. We discover the same kind of variation, relative to this time scale, in two entirely different sets of events (that one happens to be terrestrial, the other celestial, seems to me wholly irrelevant). Now usually the discovery of such precisely similar variations leads us to suspect the existence of a causal relationship between the events—in this case, according to Toulmin, we would have to say that the rate of revolution of the Jovian satellites is affected by the seasons of the terrestrial year[1]; and this, we are led to believe, would simply be a ridiculous hypothesis: "who will want to concede" its possibility? "We clearly do better" to abandon the standard of temporal measurement which leads to this absurd conclusion.

But surely there is more to the case than this: surely there are good reasons, which ought to be brought out, why we would not want to concede the possibility of a causal relation between the terrestrial seasons and the motions of the Jovian satellites. There is, after all, nothing logically impossible about entertaining such a hypothesis: the observed precise similarity of variations *could*, just as much as the observed irregularities in the swing of the pendulum, be admitted as "a genuine phenomenon calling for a physical explanation." Nor would the discovery of still further cases of events showing variations precisely similar to these make logically untenable the hypothesis of a causal relationship between all the events concerned.[2] That is, even after the

[1] Actually, all we would have to admit (and even that, only on the acceptance of Mill's criteria of causal explanation) would be that there is some interrelation of some kind between them: perhaps the motions of the Jovian satellites affect the seasons of the year; or perhaps some third factor is the cause of both variations.

[2] One might expect that, if the variations are due to our choice of standard, precisely similar variations should exist in *all* physical events. In practice,

discovery of the irregularities in the motions of the Jovian satellites relative to the sun's motion, and even after the discovery of precisely similar variations in other, apparently independent events, we could still continue to consider the sun's motion as temporally uniform, and so as suitable to serve as a standard of temporal measurement, and attempt to find some way of explaining the variations.

What, then, is the reason why, in the case given by Toulmin, the decision is made to reject the possibility of such interaction? Fundamentally, the reason is that, within the framework of current physical theory, there is no way to explain how such interaction might take place. Even this reason, of course, is not completely compelling: we might still be willing to admit that there might be some kind of "force" hitherto unrecognized that is responsible for the variations. And we shall see, below, that sometimes it might be convenient to take this approach. But in the case given by Toulmin, the reasoning might be summarized in the following procedural principle: *If, relative to a particular standard of temporal measurement, precisely similar variations are found in a number of temporal processes, and if current physical theory does not provide the possibility of there being a causal relationship between those processes, then we are to attribute those observed irregularities to the standard rather than to those processes.* When we discover the same variation in the motions of the Jovian satellites relative to the sun's motion as standard of temporal uniformity, as in the motion of the pendulum relative to the same standard, and when no way is found in current physical theory of relating those events, then we are, according to this principle, to consider those variations as due to our choice of standard, rather than as being "a genuine phenomenon calling for a physical explanation."

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however, the determination that such systematic irregularities occur, or that they are "precisely similar," is not so simple; in the case given by Toulmin, for example, other systematic irregularities overlie the one attributed to the choice of temporal standard: *e.g.*, in the seventeenth century, it was noticed that the times of eclipse of the Jovian satellites showed a periodic deviation from the predicted times dependent on the distance of Jupiter from the earth. This systematic deviation was attributed to the finite velocity of light, and on this hypothesis Römer made his original measurement of that velocity (1675). It is not always so possible to disentangle the various observed deviations from the expected, however. Still, when the number of events begin to multiply in which precisely, or almost precisely, the same systematic variations of process occur, we are tempted to look rather to our standard than to the rest of the universe for an explanation of the variations. But it must be understood that this reasoning is not logically compelling.

This procedural principle can also be stated, not as a principle of the rejection of time scales, but rather as a principle of the acceptance of new time scales: *So choose the standard of temporal measurement that no systematic irregularities will occur in nature which are not explained in terms of current physical theory.* I shall call this principle, in either of these forms, *P*. It should be noted that *P* can be rephrased to refer to standards of spatial rather than of temporal measurement, or to refer to standards of measurement in general.

Now Toulmin claims that the reasoning involved in his example is clarified by bringing in the expression "mathematical ideal," and that, understood as mathematical ideals, Newton's notions of absolute space and absolute time make scientific sense. But the exact role of these ideals in the reasoning about time scales is not clear. On the one hand, one gets the impression throughout the paper that the mathematical ideals, "absolute space" and "absolute time," serve as standards by which we decide which time scales are satisfactory and which are not. For example:

> In dynamics as in geometry it can always be asked how accurately in fact particular figures or measures realize our ideals, whether of "right lines and circles" or of "absolute, true" time and space. Sensible standards and measures of position, velocity, and lapse of time will possess more or less in fact the characteristics which the variables x, y, z, t, and their derivatives possess in the theory from their own nature [I, 18].

On the other hand, in the example quoted above, he seems to be saying that we arrive at the necessity of using mathematical ideals *after* we have found at least one sensible standard to be unsatisfactory: in place of that sensible standard we take "for theoretical purposes a dynamical ideal." This statement would lead us to suppose that we decided that standard to be unsatisfactory on grounds other than its failure to conform to the mathematical ideal.

Toulmin nowhere makes clear what are "the characteristics which the variables x, y, z, t, and their derivatives possess in the theory" of Newton. Nor does he tell us how those formal characteristics function as "ideals" governing the choice of a particular sensible standard—if this is how he supposes them to function in that theory. If, on the contrary, he supposes them to be arrived at by a decision to reject some (or all) sensible standards, the process by which this takes place is left equally unexplained. But fortunately, it is unnecessary to try to determine which of these interpretations is really Toulmin's meaning; for in neither case is the expression "mathematical ideal" enlightening about the reasoning involved in his example.

We have seen that the logic of the choice made in Toulmin's example, about whether to reject a certain time scale, is brought out by the procedural principle *P*. That is, *P* summarizes the reasons—provides the standard—for selecting and rejecting time scales. Now is there any useful sense in which *P* can be said either to be, or to lead to the adoption of, a "mathematical ideal"? For if the mathematical ideal "absolute time" functions as a standard for determining whether a sensible standard of time measurement is satisfactory, then it should amount to *P*; and if *P* leads to the adoption of the mathematical ideal "absolute time," then we should be able to understand how this comes about.

Can *P* be said, in any useful sense, to be either "mathematical" or "ideal"? One might be led to call it "mathematical" by thinking along lines like these: we decide whether to change time scales by seeing whether we can account for the observed variations in terms of the laws of our physical theory; but these laws are derivable, in Newtonian physics, from certain axioms; and the fundamental variables of the system are defined through the ways in which they are related in these axioms. Hence, ultimately, it is the formal characteristics of the variables, as set down in the axioms, that lead us to decide what kind of time scale to look for.

But this reasoning would overlook some important points. First, the laws used to determine whether or not to attribute the variations to the time scale are not pure, uninterpreted mathematical formulae; they are interpreted, not merely "formal." Furthermore, it is not merely—as Toulmin, on this interpretation, suggests it is—the characteristics of the time variable that tell us what kind of time scale to try to find; it is the laws of the system as a whole. In fact, as I will point out later, time in Newtonian physics can have no causal efficacy; and since, in trying to decide on a time scale, we are concerned to see if certain observed variations can be explained physically, the characteristics of the time variable itself are not at all central to the problem of choosing a time scale.

I can therefore see no good argument for referring to *P*, the reasoning behind the selection and rejection of time scales, as "mathematical," or as dependent purely on the "formal characteristics" of the variable *t* and its derivatives. That reasoning makes use of the characteristics of the mathematical variables, since it is concerned with the physical laws; but it would be entirely misleading to say that that reasoning depends on nothing but those formal characteristics—and still worse to say that that reasoning depends on nothing but the formal characteristics of the time variable *t*.

Nor can we say, without careful qualification, that *P* functions as an "ideal" in terms of which we choose our standards of temporal measurement: if we call *P* an ideal, then we must remember that it is an ideal in the sense of being a standard for choosing time scales; it is *not* an ideal in the sense of being *itself* a time scale, but more perfect than ordinary ones, to which all ordinary time scales "approximate." And this brings us back to the fault in the other possible interpretation of Toulmin's "mathematical ideals"—that we decide to adopt them after we have reasoned according to *P*. For in deciding to consider the sun's motion as temporally irregular (that is, as no longer serving, without qualification, as our standard of temporal measurement), we are not "abandoning the previous 'vulgar' measure of time and taking in its place for theoretical purposes a dynamical ideal." What we are doing is taking as our standard another empirical one—albeit, no doubt, a highly complex one—which takes into consideration the various systematic irregularities which would result from taking *any one* of a number of candidates (pendulum, sun's motion, and so forth) as our sole standard, and so balances our particular standards as to cancel, as much as possible, the resultant systematic irregularities.[3] And we must be especially careful in speaking of "so nearly uniform motions" which are "the closest physical realization" of our ideal; for such locutions make it all too easy to slip into thinking of the ideal as a perfectly uniform, nonphysical motion, and to forget, as Toulmin seems sometimes to have forgotten, what kind of a thing the standard really is.

Thus it does not seem very profitable to speak of the reasons for abandoning a temporal scale, and for choosing a new one, in terms of a fulfillment of "mathematical ideals." For, by saying that the "formal characteristics" of the Newtonian mathematical system serve as

[3] People are often puzzled by questions like this: how can we say, of an atomic maser clock—the best clock we have—that it is accurate to so many seconds per ten thousand years (doesn't that imply our comparing it with a more perfect clock?)? The answer, built into physical theory, is not that we compare its time measurement with a perfect but nonempirical time scale, but rather that, if we did not say this, then many other things—perhaps even all other things—would be found to happen with a slight, and in all cases exactly similar, irregularity. And we can use other particular standards, like pendulums, because we know, in each case, what qualifications to make in order that, within the limits of present empirical knowledge, no systematic irregularities will occur in nature that do not have a physical explanation. It should be pointed out, incidentally, that change or modification of time scale has, according to the analysis I have given, nothing to do with passing from a "vulgar" to a nonvulgar scale.

"mathematical ideals" in terms of which we are, according to that system, to choose our empirical standards of temporal measurement, Toulmin obscures the reasoning behind the choice of temporal scale, and even misleads himself into a Platonic interpretation of such "ideals." Nor is it any better to say that, when we decide to abandon a time scale, we replace it by a perfect nonempirical one in the form of our "mathematical ideals."

In general, observance of *P* has great advantages for science. Science has always aimed at accounting for as much of experience as possible in terms of as few independent hypotheses as possible. And *P* is essentially a technique for avoiding the necessity of introducing any new independent hypotheses or modifying old ones. Looked at in this way, the reasoning summarized in *P* becomes just a special application of the principle of "simplicity" in science.

But it may nevertheless be true that, in the long run, a better—that is, a simpler, more usable—account of phenomena might be given if we admit the existence, say, of the variations observed in our example, even at the expense of introducing a new mechanical principle or of modifying some of our old ones; for such change might well make theoretically dispensable a number of other laws or concepts previously thought fundamental and independent. A truly undogmatic, scientific attitude must admit at least the possibility that "the rate of circulation of these satellites around Jupiter is affected by the seasons" of the terrestrial year.

But when faced with the possibility of adding to, dropping, or modifying some of its fundamental principles, science is naturally, and for good reasons, conservative. Usually, in fact, whether conditions require that *P* should or should not be observed is not entirely clear; and in such cases science adopts a "wait-and-see" attitude. The following discussion will illustrate three circumstances—often closely conjoined in practice—in which such a tentative attitude may be adopted.

(1) Although the observed irregularities may actually have been explained in terms of current theory, that explanation may not be wholly satisfactory; in such cases we leave open the possibility that we may yet have to modify our time scale. For example, the spectra of distant galaxies show a shift toward the red end of the spectrum: the emitted light has a longer wave length, a lower frequency, than normal; and this irregularity is systematic, in the sense that, statistically speaking, the dimmer (and therefore, again statistically speaking, the farther) the galaxy, the greater the shift to the red. Here, then, we have a case of "precisely similar" variations to account for. Now these

variations can be explained in terms of the Doppler Effect: the galaxies are moving away from us, and the farther the galaxy, the greater the velocity of recession. But many prominent scientists have not considered this explanation entirely satisfactory for many reasons (for example, it involves an enormous extrapolation of the Doppler Effect from terrestrial cases to a cosmic scale; it requires the galaxies to possess, relative to one another, velocities which approach and, at great enough distances, can even be expected to surpass the velocity of light). And if we have to abandon this explanation, we may have to try attributing the irregularity to our present time and distance scales rather than to the motions of the galaxies themselves.

(2) In cases in which everything seems to indicate the advisability of a modification of time scale, there may be no clear way forthcoming of so choosing a new time scale, or of modifying our old one, that all unexplainable irregularities will be eliminated completely: all proposed new scales may, for instance, take care of the irregularities in question, but may introduce new ones of their own. In such cases again, the final decision as to a course of action is left open. And even if the new time scale seems free of such irregularities, there is no way of telling immediately whether it is to be trusted or not; for we must always be prepared to discover, relative to any time scale we adopt, systematic irregularities which we had not noticed before. (There is, of course, no reason why this *must* happen.) Here again we can see the virtue of the careful, tentative approach in the employment of *P*.

(3) Even when a new time scale is available which does not appear to introduce any unexplainable systematic irregularities, that new scale may itself require serious modifications of current theory. This will clearly be the case if (as in the example of the Doppler Effect given above) an explanation in terms of current theory is rejected as unsatisfactory; but it may not always be immediately clear whether such modifications of theory will be necessitated or not. For example, to attribute the supposed expansion of the universe to our choice of time scale may entail abandonment of the hypothesis that the Doppler Effect holds throughout the universe, or else that the frequency of light does not change with time. However, at least one scientist has been willing to make such sacrifices: one of the primary purposes of E. A. Milne, in his construction of his "tau-time" scale, was to eliminate the necessity of assuming that the universe is expanding without introducing any other unexplained irregularities. But because Milne's approach—apart from the many difficulties in the details of his theory—requires the abandonment of some principles that have

proved of great explanatory value in science, it has been viewed with extreme caution (sometimes, unfairly, with worse). And this is only right: it would be dangerous to throw over the cumulative advances of years in favor of a radically new and relatively untried system which is designed to take care of only a small part of the total number of facts which the older theory has proved capable of handling. And besides, it may be possible to remove all the difficulties in the current explanation of the red-shift; for such explanations, too, cannot always be presented immediately with all possible clarity. But the fact remains that Milne (for example) may be right, at least in his approach: such a radical change of time scale, even with its attendant modifications of current physical laws, might in the long run provide a better account of experience than we now have.

And so the problem of the selection of and adherence to a particular standard of temporal measurement is far more complicated than Toulmin has suggested. And once we have seen that the issue with which he is concerned involves a procedural principle regarding acceptance or rejection of such standards, and that *in some cases it might be convenient to violate that principle*, we can see that his apology for Newton's use of "absolute time" cannot be successful.

For in Newtonian physics it was an ultimate and unquestioned dogma that all physical events were to be accounted for solely in terms of the properties and interactions of masses; and it was for *this* reason, and not for the good reasons discussed above, that in Newtonian physics P was to be observed—and observed in all cases, without exception. Whenever a systematic irregularity was encountered which could not be accounted for in terms of the specified properties and interactions of masses, that irregularity was to be treated as due solely to our choice of reference frame. Space and time thus became for Newtonian physics convenient dumping-grounds to which could be relegated all observations which did not fit this fundamental dogma.[4]

Thus, even if Newton (or any other scientist) used a concept, which

[4] It follows from this dogma that neither the irregularities nor any other phenomena could be said to be due to the spatial or temporal location, or to the spatial orientation, of the events concerned; only masses could have causal efficacy. This requirement of spatial and temporal homogeneity and isotropy imposed further restrictions on what could count as an explanation, as well as on the concepts of space and time, in Newtonian physics. Any complete account of the Newtonian concepts of absolute space and time would have to exhibit those concepts as hybrids of many separate ideas: I would not like to claim that P is *all* that was meaningful in the Newtonian notion of the "absoluteness" of absolute time.

he called "absolute time," which amounted in practice to adherence to the procedural principle *P* as a fundamental and always-to-be-obeyed rule, such adherence would still be open to severe criticism. For though it might not be adherence to a meaningless metaphysical principle, nevertheless there can arise in science important cases in which adherence to the principle might be unwarranted in the light of the fact that rejection of it might produce a better system. Hence Toulmin's apology for Newton's work has failed to bring out the remaining anti-scientific element of the "absolute space and time" doctrine.

It would be futile to argue that metaphysicians who talked about "substance" were anticipating or struggling toward a scientific procedural principle but mistaking it for a proposition about "reality": this *may* be true of the "absolute time" doctrine, but may not be true of other such notions. Still, a parallel can be drawn between the old arguments that there "must" be a substratum of change, and later scientific conservation principles and principles about the continuity of physical processes; and it might be argued that those principles, too, are procedural, in the same way that *P*, which may be the meaningful core or part of the meaningful core of the "absolute time" doctrine, is a procedural principle. And again, though the "principle of the continuity of physical processes" seems to have been held as a dogma by classical physics, its hold has been greatly weakened by the quantum theory. Only in this century have scientists begun to realize the width and depth of the options open to them in the acceptance or rejection of such principles.

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CRITICAL REASONS

PHILOSOPHICAL problems have a way of reappearing, though perhaps somewhat disguised in their new formulations. This is why we honor a philosopher for his astute sense of a basic problem even when we would not now choose to present or discuss the problem as it was originally presented or discussed. The terms, the context, and the proposed solution of Kant's antinomy of the judgment of taste seem remote from contemporary aesthetic discourse, but the basic difficulty of how we are to reconcile the singularity of the judgment of taste with its claim to objective correctness is still with us.

Today it would be more usual to regard the judgment of taste—the judgment of aesthetic appraisal—as expressing a commendation rather than as ascribing a quality, but this revision amounts to less than might be supposed, if commendations make an implicit claim to justification. So long as such a claim is involved, the singularity of the judgment of taste presents a problem. How can one seek to justify a commendation except by presenting reasons? But how can one present reasons without thereby assuming or implying a generality incompatible with the view that the judgment of taste is concerned with an individual object considered *as* individual?

One might, of course, seek to avoid the difficulty by denying that the judgment of taste has the singularity that Kant and others have ascribed to it. Something of a case can be made out here by showing the extent to which, in actual critical practice, works of art are commended as good of their kind. "That's a fine Ming vase." "That's a good English poem in the romantic style." It may sensibly be claimed that it is largely through funded experience of works of a certain kind or class that we develop appropriate critical expectations about what can be achieved along a certain line. Then, too, there evidently are general norms. But when everything has been said that should be said to bring out these points, we may consider that we still have to reckon with the singularity of the judgment of taste. This judgment must ultimately be based, not simply on direct inspection of the individual work of art, but on the kind of inspection that takes account of its individuality. Knowledge of "other things of the same sort" is perhaps chiefly useful in bringing out this very individuality. The distinctive character of Chartres Cathedral is best recognized by one who has a wide acquaintance with other Gothic structures.

One could avoid the problem posed in the antinomy of taste by the alternative device of denying that judgments of aesthetic appraisal make a claim to justification. This would be to say that "reasons," offered in connection with such appraisals, cite causes rather than grounds. In support of this view one might point out the extent to which, in practical criticism, the critic appeals to particular qualities of the work of art rather than to general principles. If, then, the intent of the critic is to share his experiences with the reader, this activity seems rather different from that of marshalling evidence. But though it is important to recognize and to acknowledge this aspect of critical discourse, we may still consider that critical reasons are more like justifications than explanations. To the question "Why do you like this picture?" the answer "Because of the delicacy of the color gradation" may not seem completely satisfactory as a justification. But notice how different it is from "Because it reminds me of my grandmother." This latter, though surely relevant as causal explanation of response, is ruled out in ordinary contexts of critical discourse, written and oral.

If the effect of these considerations should be to confirm us in the view that the problem presented in the antinomy of taste is still with us, it may be worthwhile to cast about for a solution. In an influential paper, "Critical Communication,"[1] Arnold Isenberg sets forth a claim which, I believe, carries us some distance in this direction. I shall hope to explain why I think it does not carry us all the way and to offer what I believe to be a necessary modification.

Isenberg takes as his point of departure a theory, widely held in spite of its deficiencies, that divides the critical process into three parts. There is the verdict (V): "This picture or poem is good. . . ." There is a reason (R): ". . . because it has such and such quality. . . ." There is a general statement or norm (N) ". . . and any work that has that quality is *pro tanto* good. . . ." He argues convincingly that it must be a mistake to suppose that, in offering a reason in support of an appraisal, a critic is committed to a generalization of the form ⌜Any work of art that has that quality will also be *pro tanto* good.⌝ It only seems to be the case that such a generalization is involved. He suggests that we rid ourselves of this erroneous impression by remembering that the function of critical discourse is to mediate the work of art to the reader and that, in consequence, reading such discourse other than in the

[1] *Philosophical Review*, LXIII (1949), 330-344. Reprinted in *Aesthetics and Language*, New York, 1954.

presence or with direct recollection of the work is a senseless employment. If, however, we relate the critical discourse to the work, we recognize that terms used by the critic in his statement of reasons—terms such as "assonance" or "wavelike contour"—always refer to a particular assonance in a particular poetic line, or to a particular wavelike contour in a particular picture.

All this seems to me to be as sound as it is important. But surely the conclusion to be drawn is that, *in so far as this particularity of reference is taken account of*, the reason does not admit of the supposed generalization. Otherwise expressed, what the critic is claiming is that specific quality Q is a good-making quality in work of art W. Because of the particularity of the refernce, no generalization of the form ⌜Q-like qualities are, or tend to be, good-making qualities in works of art⌝ follows. Unfortunately, however, what Isenberg says is that the generalization in question does not follow because the critic never really claims that the work is good for the reasons mentioned.[2] This is a puzzling and, on the face of it, implausible assertion, for surely the critic naturally supposes that he *is* making just such a claim.

It would be nice to believe that the difficulty here is merely verbal and arises from a narrower or broader interpretation of "reason mentioned." This would allow us to say that if "reason mentioned" is to be understood apart from the experience of the work of art, then the critic is *not* making the claim that the work is good for the reason mentioned, but that if "reason mentioned" is filled out by the appropriate experience or recollection of the particular work, then the critic *is* making the claim. But it is evident that Isenberg is not content merely to say that the critic's reasons cannot be *understood* except by reference to the work of art; he feels constrained to acknowledge that, even as so understood, the critic's reasons cannot function as justifications for the critic's appraisal. Therefore, some other function must be found for these reasons, and this is said to be that of modifying the way the work of art is perceived.

Thus Isenberg's position is as follows. The critic may suppose that he is offering reasons to support an artistic appraisal but, in fact, he is using language in such a way as to modify the reader's aesthetic apprehension of the object. This may (or, again, it may not) result in a concurrence of appraisal as between the critic and his reader. If Isenberg is right, the most that a critic ever does is to evoke agreement,

[2] "The critic is not committed to the general claim that the quality named Q is valuable because he never makes the particular claim that the work is good in virtue of the presence of Q." *Ibid.*, p. 338.

and if he thinks that he is doing something other or more than this, he is in error. I find this conclusion implausible because I find it hard to believe that critics can be so radically mistaken in what they customarily suppose themselves to be doing.

What drives Isenberg to this implausible conclusion is the intellectual pressure of the following considerations. Reasons can function as genuine justifying grounds for appraisals only if they are general (quality Q is, as such, a good-making quality). If reasons are particular they are perfectly arbitrary. But reasons *are* particular, so they must be arbitrary. Yet the activity of the critic in giving reasons does not seem to be gratuitous or useless. Therefore, some function other than that of justifying appraisals must be found for it. This function is that of seeking to evoke agreement in appraisal by enstating communion in apprehension.[3] In what follows, I shall attempt to show that there is a more plausible path out of this dilemma.

Isenberg is surely correct with respect to the point he is most intent on establishing. This is that what the critic discerns is the good-making influence of a quality or characteristic *as this displays itself in an individual work of art*. The critical judgment really is: goodness here because of this. I think Isenberg is also correct in supposing that some reference to a norm or standard must be involved if reasons are to be justifying reasons. He is mistaken, though, in thinking that the normative assumption, were such an assumption involved, would be of the form ⌜Quality Q is, as such, a good-making quality⌝ or ⌜Any work of art that has quality Q will be *pro tanto* good.⌝ I wish to suggest that normative presuppositions are not generalizations of or from particular reasons. On the contrary, such presuppositions function as regulative principles guiding choice in the selection of particular reasons.

A particular reason, offered in support of an appraisal, must recommend itself as in some way appropriate for its office. What characteristics must a reason have in order to make a decent claim to the requisite appropriateness? First of all, the quality or characteristic mentioned in the reason must be discoverable in the work of art. But this, though necessary, is far from sufficient. In addition, the reason must be such as to have some reference, however indirect, to a norm. Now it is the combination of these requirements that has proved puzzling. We seem to be trapped in a dilemma. For if the reason is particular enough to be evidently relevant to the art work, it is too

[3] ". . . so long as we have no alternative interpretation of the import and function of R we must assume *either* that R is perfectly arbitrary *or* that it presupposes and depends on some general claim." *Ibid.*, p. 335.

particular to admit of the generalization: every other art work that has this quality will also be *pro tanto* good. On the other hand, if the reason is general enough to function plausibly as itself a norm (this picture is good because it is organized, integrated, unified) the reason is unsatisfactory because its relevance to the art work is uncertain. Presented with such a reason, we shall be likely to say, "Well, if it is unified, it is, no doubt, so far good, but *is* it unified?" Thus it would appear that we are presented with an unsatisfactory forced choice between reasons relevant to the art work but lacking normative import or reference, and reasons having such import but doubtful as relevant to the art work.

Now it seems to me that there must be some way of avoiding this impasse. The impression of the impasse is produced by scrutinizing the critical reason in isolation from its context in critical discourse and thus neglecting its functional character. It is the function of a reason to mediate between an individual art work and a general norm. The claim I wish to put forward is that a plausible reason owes its plausibility to the fact that it has undergone a twofold process of selection. It has been selected on the basis of the consideration of the individual work of art *and* on the basis of the assumption of some general norm. In saying this I am not claiming that the competent critic always is, or needs to be, clearly conscious that he is following such a procedure. The critic's "sense" of the plausible reason can be fully effective in practice without the accompaniment of any explanatory theory.

The claim that the critic's choice of a reason is influenced by his apprehension of the work of art requires no clarification. What needs to be clarified is the claim that this choice is also influenced by normative presuppositions and that norms operate as regulative principles guiding such choice. The notion of a norm as a regulative principle requires to be explained because we often think of norms not as presuppositions but as tests. There is a puzzle about the role of norms in practical criticism that needs to be cleared up. Though it is evident that responsible criticism requires the assumption of artistic norms, since otherwise we have only the anarchy of purely personal impressions and predilections, it is equally evident that general norms do not lend themselves to simple and direct application as tests and rules. Critical appraisal "by rule" seems to be just about as unpromising as artistic creation "by rule." So it seems that norms are essential and yet, somehow, useless. We must, I suggest, give up thinking of artistic norms as tests, after the analogy of scientific tests suitable for determining the presence or absence of this or that, and think of them instead

as regulative presuppositions or principles. The use of a norm by a competent and perceptive practicing critic is as different as possible from the use of, shall we say, litmus paper. A norm is, indeed, a requirement but it is relatively worthless as a test. It is, I think, the awareness of this that makes the critic impatient and intemperate when asked to state the norms by which he formulates his appraisals. He is as likely as not to say that he has no traffic with norms—he does it by smell—though, of course, he will resent the suggestion that he is without artistic standards. In short, he has assumptions, indeed principles, but he does not have rules and, in any simple and straightforward sense, he does not have tests. Whatever the empirical derivation of norms, they are, methodologically, ultimate. Accordingly, they must recommend themselves on their own merits; they must be and seem plausible simply as such. To have this plausibility they must be general *to the point of obviousness*, and this generality makes them unsuitable as practical tests. So we shall get along better if we recognize that norms operate indirectly as regulative principles governing critical perception and critical choice of justifying reasons rather than directly as criteria for particular appraisals.

In order to develop this view further it will be useful to mention some commonly recognized artistic norms. All of these, I trust, have the necessary requirement of obviousness. First of all, unity: a satisfactory work of art must be unified, integrated, organized. Second, complexity: a satisfactory work of art must be sufficiently complex to hold and to reward attention and to invite renewed attention. Third, generative powers: a satisfactory work of art must be such that the parts directly and explicitly presented can interact with one another to generate emergent regional qualities. To put the matter negatively, it is undesirable for an art work to be disorganized, banal, or dead. It will be noted that these norms are too general to serve helpfully as criteria for particular judgments of artistic appraisal. Their function is to serve as criteria for the good selection of reasons (or the selection of good reasons) to be presented in support of artistic appraisals.

As I have said, a reason offered in support of such an appraisal must, in order to function plausibly as a reason, have two kinds of relevance—relevance to the art work and relevance to a norm. It will not be sufficient that the qualities mentioned in the reasons can be found in the work, for it will always be appropriate to ask: what do these qualities *do* for the poem as a whole, or for the stanza, or for the poetic line? If, on many occasions, we do not find it necessary to ask this question but, instead, look directly for what it does, this is because the critic

has been guided in his selection of reasons by commonly accepted normative presuppositions. No responsible critic would, for instance, commend a painting because it presented a landscape, or because it contained a good deal of blue, but he might commend it on the ground of the subtle gradation in the color arrangement. In such a case, he is not either presupposing or suggesting that this is a good thing in all paintings or that all good paintings must have this. What he is asserting is: *this* color arrangement is good *here*. But the goodness of the color arrangement there is a matter of what, in that situation, it *does*. Perhaps it contributes significantly to the integration of the pictorial design. Perhaps it generates a pervasive regional quality of lyrical grace.

In short, what I am claiming is that a plausible reason mentions a characteristic that is present *and* operative. The evidence for this is that, with reference to any such characteristic or quality, it is always appropriate to ask two questions. Is it discoverable in the art work? What does its presence there do for the work as a whole or for some distinguishable subpart thereof? The answer to this latter question is not the all-too-general reply, "It makes it good," but something more specific, as, for example, "It introduces a multiplicity of reference that gives symbolic depth to the poem (or stanza)." Behind such a reply lies the normative assumption that, other things being equal, complexity is a good-making characteristic.

There is nothing odd, peculiar, or contradictory in the circumstance that the same critic may commend different art works—for instance, different pictures, for different reasons—in one case mentioning the delicate gradation of color and, in another, the bold dynamic contrast. The operation of common norms as regulative principles guiding choice in the selection of reasons is compatible with great diversity of reasons. But this is as it should be, for a reason must be relevant to a particular work of art, and works of art differ one from another.

Reasons, in order to be reasons, do not have to be conclusive reasons. What makes a reason a justifying reason is the plausibility of its *claim to consideration* rather than its actual persuasive power. There are many reasons (explanations) why reasons (grounds) may not, on occasion, be effective in securing agreement in artistic appraisal. Norms commonly accepted may yet be differently weighted in point of importance. Then, too, there is difference of opinion about the suitability and the importance of special norms in special fields, such as literature and architecture. Also, of course, works of art can be very complex and so there is room for uncertainty and dispute in the ascription of qualities. A consideration of these and other matters would be necessary for any

extensive theory of criticism but they are not directly relevant to the main point of this paper, which is concerned with an explanation of what seems to be the puzzling nature of critical reasons.

Critical reasons have been found puzzling because, if they are to be relevant to the art work, it seems they must be particular, but if they are to be justifying reasons, it seems they must be general. How can they be both? I have sought to dispel this puzzle by suggesting that we give up scrutinizing the reason in isolation and consider instead its mediating function in critical discourse. It is the office of such a reason to mediate between a particular work of art and a general norm or norms. The full import of a characteristic critical reason can be laid out as follows. This quality makes for goodness in this individual work of art because, as operative in this distinctive manner here, it makes for the fulfillment of some general normative requirement such, for example, as unity, or complexity, or generative vitality. There is both particularity of reference and generality of reference, and this is not surprising if, as I suggest, the selection of the reason has been influenced both by the apprehension of the art work and the presupposition of the norm. The reason has, in fact, two parents and it provides a bond between them.

The judgment of taste is a singular judgment in that it has the form ⌜This work of art, considered as individual work of art, is good.⌝ But the judgment of taste is not simply an expression of personal impression. The reasons offered in support of the appraisal can claim to be justifications on the basis of normative assumptions. To understand this all we have to recognize is what, I think, we really do recognize—namely, that individual works of art have *individual and distinctive ways* of satisfying commonly accepted artistic norms.

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FREGE'S ONTOLOGY

IN A recent reply[1] to Professor Gustav Bergmann's article[2] dealing with Frege's ontological commitments Mr. E. D. Klemke raises a point to which I would like to add comment.

Klemke asks whether a concept (*Begriff*) could not be the sense (*Sinn*) of a class name. He claims that though he was unable to find a definite rejection of this identification in Frege's writings, such a rejection would be reasonable on the following grounds: (1) Frege ". . . insists that a term *expresses* its sense and *stands for* (refers to, designates) its reference. He never says that a concept is what is expressed by a predicate-term." (2) Since predicate expressions do have reference for Frege, there is no reason to identify their sense and reference, nor to deny them a sense. (2) is argued on the ground that it is a more consistent rendering of Frege with regard to his distinction between the sense and reference of an expression.

The question here considered can be definitely answered. Through the kind permission of Dr. G. Hasenjaeger of the *Institut für mathematische Logik und Grundlagenforschung* in Münster, Germany, I was permitted to study yet unpublished papers by Frege. In a paper of 1891-1892 Frege argues that the sense of an expression *is an object*, and since, for Frege, objects and concepts are in every case to be distinguished (a distinction made consistently throughout his writings), the sense of an expression is never to be confused with a concept.

While this answers the question, it also suggests that Klemke's statement (1) is misleading. For if the sense of an expression is an object, it is reasonable to suppose that it could be the reference of a grammatical subject. Certainly this is the case, as a reading of "Über Sinn und Bedeutung" will confirm. Expressions occurring in indirect discourse have as their reference their ordinary (*gewöhnlichen*) sense. Frege writes that in those cases where an expression occurs in indirect discourse, ". . . ist es nicht erlaubt . . . einen Ausdruck durch einen anderen zu ersetzen, der dieselbe gewöhnliche Bedeutung hat, sondern nur durch einen solchen, welcher dieselbe ungerade Bedeutung, d.h., denselben

[1] "Professor Bergmann and Frege's 'Hidden Nominalism,'" *The Philosophical Review*, LXVIII (1959), 507-514.

[2] "Frege's Hidden Nominalism," *The Philosophical Review*, LXVII (1958), 437-459.

gewöhnlichen Sinn hat."[3] Whereas we do not have in natural languages a name for the sense of an expression—which might be regarded as a defect—we may refer to the sense of an expression through the simple expedient of such expressions as "the sense of x," where "x" takes expressions as values.

Klemke provides a table of ontological entities, dichotomized into those which can be referred to and those which cannot. In the former list appear objects and functions, in the latter senses and thoughts. As we have just noted that the sense of an expression is an object for Frege and can be referred to, we are forced to reject Klemke's dichotomy.

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[3] "Über Sinn und Bedeutung," *Zeitschrift für Philosophie und philosophische Kritik*, C (1892), 25-50.

DISTRIBUTION: A LAST WORD

In this note I shall ignore any philosophical objections to the doctrine of distribution considered as a semantical theory of the denotation and reference of terms. Before that doctrine need be even considered as a semantical theory, we should have to be certain that the formal rules of inference relating to distributed terms are correct; for the whole point of the semantical theory—of making the distinction between cases where a term refers to every one of the objects it denotes and cases where it refers to only some of them—is to supply a rationale and intuitive justification for the rules. If the rules are inconsistent, this task is futile.

Let us, then, state the rules quite formally. All we need to know about "distributed" in that case is what terms it applies to, namely, the subject terms of universal, and the predicate terms of negative, categoricals. Any other terms will be undistributed.

A conversion will be an inference from a categorical p to a categorical q such that

(i) the subject term of p (or q) is the predicate term of q (or p);

(ii) the quality (affirmative or negative) of p and of q is the same.

By the doctrine of distribution, the conversion is valid if and only if

(iii) no term is distributed in q unless it is distributed in p.

It is easily verified that, by these rules, "SaP" and "SiP" both have the converse "PiS"; "SeP" has the converse "PeS"; and "SoP" has no converse, because the term "S" in the predicate would have to be distributed, by (ii), and undistributed, by (iii).

An obversion will be an inference from a categorical p to a categorical q such that

(i) p and q have the same subject term, but the predicate terms of p and q are contradictories;

(ii) the quality of p is opposite to the quality of q.

By the doctrine of distribution, the obversion is valid if and only if

(iii) the subject term of q is not distributed unless that of p is.

We may easily verify—using a prime, as Keynes does, to form contradictory terms—that by these rules we may infer "SaP'" from "SeP," "SeP'" from "SaP," "SiP'" from "SeP" or from "SoP," and "SoP'" from "SaP" or from "SiP." These are all the valid obversions.

We thus get into no trouble if we use the doctrine of distribution to state necessary and sufficient conditions for a conversion or an obversion to be valid. But it has long been known that trouble does arise if we consider inversion. An inversion is an inference from a categorical p to a categorical q such that

(i) p and q have the same predicate term, but the subject terms of p and q are contradictory;

(ii) the quality of p is opposite to the quality of q.

Now, by the doctrine of distribution the inversion will be valid *only* if

(iii) the predicate term of q is not distributed unless that of p is.

And by this rule the inversion whereby we pass from "SaP" ("P" undistributed) to "S′oP" ("P" distributed) will be invalid. Yet by our rules we may validly pass from "SaP" to "SeP′," from "SeP" to "P′eS," from "P′eS" to "P′aS′," from "P′aS" to "S′iP′," and from "S′iP" to "S′oP."

This is not a decisive argument against the doctrine of distribution; for it rests upon the medieval "from first to last" rule, that if a chain of inference consists of links severally valid the whole chain is valid; and some may agree with Fitch in allowing exceptions to this rule—so that, in order to know whether a conclusion was proved, we should need to check not only whether, but also how, the premises had been proved. But this is a significant departure from the familiar idea of formal proof, and I do not know if the friends of distribution would be willing to make the departure. Anyhow, they must choose between the "from first to last" rule and the doctrine of distribution.

Neville Keynes deals with this old crux for the doctrine of distribution by saying that "S′oP" follows not from "SaP" alone, but from that *plus* a tacit existential premise "not everything is P" in which "P" would be distributed (*Formal Logic*, 4th ed., p. 139 f.). But now consider the following interpretation of the four traditional categorical forms:

"SaP" is read as

"$S = \wedge \cdot P = \wedge \cdot v \cdot S = V \cdot P = V \cdot v \cdot S \neq \wedge \cdot P \neq V \cdot S \subset P$."

"SeP" is read as

"$S = \wedge \cdot P = V \cdot v \cdot S = V \cdot P = \wedge \cdot v \cdot S \neq \wedge \cdot P \neq \wedge \cdot S \cap P = \wedge$."

"SiP" is read as

"$S = \wedge \cdot P \neq V \cdot v \cdot S \neq V \cdot P = \wedge \cdot v \cdot S \cap P \neq \wedge$."

"SoP" is read as

"$S = \wedge \cdot P \neq \wedge \cdot v \cdot S \neq V \cdot P = V \cdot v \cdot \sim (S \subset P)$."[1]

[1] This interpretation was already proposed by Jaśkowski in *Studia Societatis*

This interpretation preserves the square of opposition, and all and only those valid syllogisms which are traditionally so regarded. It renders valid all and only those conversions, and (if we have negative terms) all and only those obversions, which the doctrine of distribution would make out to be valid. It also contains the Łukasiewicz axioms "SaS" and "SiS." The universe is assumed to be nonempty, so that $V \neq \wedge$, but the terms may be empty and may be universal.

On this interpretation the inference from "SaP" to "S'oP" *is* a valid inference. Moreover, to exclude Keynes's defense, no added premise is needed; and the great unnaturalness of the interpretation is irrelevant. For the question is whether the doctrine of distribution affords a formal test of validity. By constructing a system in which the usual relations of categoricals are maintained, but nevertheless an inference condemned by the doctrine of distribution is valid, I have shown that the doctrine of distribution is useless even as a mechanical test of validity.

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Scientiarum Torunensis, Section A, Bd. 2 Heft 2 (1950), 77-90 (reviewed in the *Journal of Symbolic Logic*, XVII, 1952, 268). A similar interpretation was proposed by H. B. Smith in the *Journal of Philosophy*, XXI (1924), 631-633. I owe these references to Professor Alonzo Church.

BOOK REVIEWS

THE USES OF ARGUMENT. By STEPHEN TOULMIN. New York, Cambridge University Press, 1958. Pp. viii, 264. $4.50.

Suppose someone asserts that Smith's car is a four-cylindered one and, upon being challenged, supports his assertion with the further factual information that Smith's car is a Volkswagen. In this episode are illustrated three of the primary features of arguments discriminated in the analysis or "layout" of arguments presented by Stephen Toulmin in this book. They are (1) the *data* or grounds of the assertion (it is a Volkswagen), (2) the *conclusion* or assertion itself (it has four cylinders), and (3), indicated only implicitly, the rule, principle, or *warrant* that authorizes the step from the data to the conclusion (a Volkswagen may be taken to have four cylinders).

Of the further features discriminated in the analysis of typical arguments, only one needs to be noticed here. Given the data and the warrant in the above argument the conclusion necessarily follows. To challenge the conclusion is then, in effect, to challenge either the data or the warrant in the argument; and in the latter case the challenge is met by adducing further information, further data, but this time data that will meet the challenge to the warrant and gain its acceptance. This further information may be from any of a wide variety of sources. One may draw upon memory of past examinations of cars of this make, the history of this particular make of car, the published engineering specifications of its motor, and so on. Considerations of this sort, introduced to support the warrant of an argument in the face of challenge, constitute a fourth important feature of an argument and are labelled collectively the *backing* of the warrant.

With this vocabulary Toulmin proceeds to draw a distinction of the greatest importance between two types of arguments, and thence to elaborate the central thesis of the book. The distinction is between *analytic* and *substantial* arguments and depends, neglecting a few non-essential refinements, on whether or not in any given argument the data and the backing together entail the conclusion. In analytic arguments they do and in substantial arguments they do not.

The thesis is that logic and (through it) philosophy have been suffering from a preoccupation with and excessive respect for analytic arguments. Logicians have devoted themselves almost exclusively to

the study of analytic arguments—arguments in which, since the backing and data do entail the conclusion, no warrant but a principle of entailment is needed (if that can be said to be needed at all) to justify the passage from one's combined grounds, in data and backing, to the conclusion. And the study of substantial arguments—arguments in which there is no such entailment relation, in which a substantial warrant *is* required in passing from data to conclusion, and in which the backing serves to *support* such a warrant rather than *replace* it—has fallen into neglect. And all this in spite of the fact that, apart from pure mathematics, it is the substantial type of argument rather than the analytic that is by far the more frequent and typical in use.

Following in the steps of the logicians, a considerable proportion of philosophers have come mistakenly to attribute to analytic arguments a logical superiority, and the effects of this attribution upon the philosophical appraisal of the substantial arguments that we do and must constantly employ in the conduct of life, in science, and in the other intellectual disciplines is disastrous. We constantly do and must employ arguments in which there can be, in the nature of the case, no analytic warrant, no entailment, between the data and the backing we can provide for our argument and the conclusion to which it leads. This is so for arguments from information at our disposal concerning the present and past to conclusions about the future. It holds likewise for arguments from the present and immediate past to conclusions concerning the more distant past. It holds for those ethical and aesthetic arguments in which we employ factual grounds in support of an evaluative or normative conclusion. It holds for any argument in which we draw a conclusion concerning someone else's mental state from knowledge of his behavior. And it holds for arguments in which a report of a perceptual experience is used in support of an allegation concerning external objects. The insistence that conclusions of any one of these types must be established if they are acceptable, by purely analytic means is illogical, frustrating, and fully capable of generating the kind of puzzles and skepticism about the basis of such conclusions that have occupied so much of the attention of modern philosophers.

The general thesis here, of the logical adequacy of substantial arguments, is not, as Toulmin is well aware, a novel one in contemporary philosophy. But it by no means follows that it is a thesis that is well understood and in no need of explanation, scrutiny, and appraisal, even on the part of those who, in the present climate of philosophical opinion, would accord it readiest assent. This book valuably provides an occasion for such scrutiny, understanding, and appraisal by advancing

a vigorous, systematic, and persuasive case for the thesis, coupled with an outline of some of its more noteworthy philosophical consequences. In the course of elaborating his argument Toulmin also presents some interesting and illuminating discussions of further topics of considerable present logical interest, such as the existential import of universal propositions, the notion of universal "premises," and, more particularly, modal terms. In addition to reprinting his earlier paper on probability, he enlarges upon the general view adopted there, extending it to apply to the notions of possibility, impossibility, and necessity.

In connection with the general thesis of the logical adequacy of substantial arguments, there is one point in addition to those emphasized in this book which is of the utmost importance in the interest of a sound appreciation of the thesis and its bearing upon various species of philosophical skepticism. This point concerns the function of factual backing in providing support for warrants in substantial arguments and the dispensability of such backing in the case of some warrants in arguments that are sound. Lurking behind this question of appraisal and dispensability will be easily recognized the familiar features of the problem of the justification of inductive conclusions and its logical kindred, the problem of the justification of value judgments, of the attribution of mental states to others, and so on. Toulmin's principal contention, in opposition to the varieties of skepticism that have been associated with these questions, is that the requirement of analyticity, which appears again and again in various skeptical arguments, is a preposterously unreasonable one. And it is not difficult to see that this must be so. When the backing of a warrant is conceived, as it is here, as a "straight-forward matter of fact" that can be checked upon by some more or less definite testing procedure by the time the warrant is employed, the requirement of analyticity (that data and backing entail conclusion) becomes a severe one to satisfy. It is one that, apart from the domain of pure mathematics, it is logically impossible to satisfy, except for a restricted class of odd, inutile, and fatuously circular arguments. For the principles and rules that are employed as major premises or warrants for our arguments are ordinarily expressions that are in a very full sense law-like in their employment and range of application. The warrant that *A*'*s* are *B* must be applicable to *A*'*s* that have not been checked in this regard, in order for the warrant to be capable of performing its ordinary service of validating conclusions about the unobserved past and present and about the future. (Otherwise it would not be possible, and would never have been possible, for any human being to infer, according to the worn

syllogism, the mortality of Socrates.) The warrant must likewise be applicable to *A's* that cannot have been checked for the reasons that they are in some sense only possible *A's*, if it is to be capable of validating contrary-to-fact conclusions about what might have been.

This is an important point. But another, related point of equal importance for the present discussion of grounds, reasons, and justfication, is that citable factual backing for every warrant is not a necessary feature of a logically adequate substantial argument. That Toulmin is not in disagreement with this latter point is indicated at two places in the book, one where he touches very briefly on the necessity of accepting "some warrants . . . provisionally without further challenge" (p. 106), and the other where he discusses types of assertion and claims to knowledge (for example, knowledge of my brother's name, or that 5 times 7 is 35), with respect to which "it does not even make sense to talk of producing grounds for one's assertion—the demand for grounds is quite out of place" (p. 242).

One of the major sources of philosophical skepticism, indeed one of the reasons for that philosophical attractiveness of analytic arguments of which Toulmin is so keenly aware is the natural presumption that, when the warrant employed in an argument is a substantial one, it is necessary that it be provided with adequate factual backing. This in turn appears to require that one be able, in order properly to justify his conclusion, to show that the backing provided is adequate. This double requirement, both features of which are commonly accepted as proper in philosophical discussions of justification, is one that is not possible—not logically possible—to satisfy. For how could one possibly satisfy it except by the use of warrants? And how could one satisfy it thus without either using the same warrant whose backing is in question and thus failing by reason of circularity, or using a different warrant and then failing by reason of a commitment to an infinite regress?

However natural this presumption about the requirement of backing may be, it is—and good sense about substantial arguments and philosophical justification requires that it be recognized as being—a philosophical prejudice, a genuine Baconian Idol. Reasoning, argument, knowledge—life, in short—is not like that. The existence of trustworthy warrants is as much a fact of life as the existence of so-called "facts" by the use of which, on occasion, we support, correct, and invalidate these warrants. Employment of these warrants in reasoning, in assertion, argument, and inference, is as much a basic skill developed in the process of growing up as a human being as is walking or talking properly, or doing sums. Nor do these skills always,

or even for the most part, require in their employment that one be capable of justifying his performance by producing factual information on call, like financial securities, to back his warrants, principles, or rules of procedure. That perspicuous philosopher David Hume was seeing something of this, though it is surely not what he meant primarily to express when, considering the warrants that he called "relations of cause and effect" and their backing, he proclaimed, in a not uncharacteristic provocative overstatement, that custom is the great guide of human life. In comment upon this proclamation, the moral of the preceding discussion of grounds and warrants, expressed in a similarly provocative overstatement, would be: The task for philosophers in meeting skepticism is not to refute Hume's contention but to understand it.

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THE UNCONSCIOUS. A Conceptual Analysis. By A. C. MACINTYRE. New York, The Humanities Press, Inc., 1958. Pp. ix, 100. $2.50.

THE CONCEPT OF MOTIVATION. By R. S. PETERS. New York, The Humanities Press, Inc., 1958. Pp. 166. $2.50.

In these volumes, recent additions to the series "Studies in Philosophical Psychology" (edited by R. F. Holland), the work of some prominent psychologists is subjected to "conceptual analysis" by two British philosophers. Mr. MacIntyre's book is a study of Freud's theory of the unconscious; Mr. Peters discusses the work of a number of motivation theorists, Freud among them. Both books are attempts to clarify concepts used in the explanation of human behavior and to elucidate the nature of such explanation. The clarification achieved is not all that could be desired, but these are, all the same, useful contributions to a field in which any clarification at all is welcome.

MacIntyre and Peters both hold that it is a mistake, and the source of much confusion, to regard explanations in terms of reasons, motives, and intentions as a species of causal explanation. And MacIntyre holds, rightly I believe, that this is so even when the reasons, intentions, and so forth, are "unconscious." It is essential to the concept of intention that, as MacIntyre puts it, an intention is "always something that could in certain circumstances be avowed" (p. 75), and it is a mistake to think that this feature of intention drops out entirely in the case of unconscious intention. MacIntyre points out that it is a feature of psychoanalysis that an explanation or interpretation of neurotic behavior, in terms of unconscious intentions, repressed desires, and so

forth, cannot be determined to be correct until the agent himself assents to it. If psychotherapy is successful the patient will finally see his past behavior as the expression of intentions, desires, emotions, and so forth, that he formerly would have denied having, and what *he* finally gives as the interpretation of his past behavior is, or is an essential part of, the criterion of what the *correct* interpretation is. The verification of a psychoanalytic explanation thus differs radically from the verification of a causal explanation.

Freud and his followers frequently speak of "the unconscious" as a hidden causal mechanism or a realm in which hidden causal forces operate. So regarded, MacIntyre argues, the existence of the unconscious is unverifiable, its alleged workings, which Freud describes in terms "more appropriate to neurophysiology" than to "the description of mental life and behavior" (p. 21), are incomprehensible, and its explanatory value is nil. But if psychoanalytic explanations are not causal, then in giving them we need be committed to no such entity. And MacIntyre argues, following a suggestion of Wittgenstein's, that Freud's use of such terms as "unconscious" and "repression" is essentially descriptive rather than explanatory; "except insofar as illuminating description may count as a kind of explanation, their place as explanatory terms is highly dubious" (p. 79). When Freud seems to be explaining phenomena, what he is generally doing, according to this view, is describing and classifying the phenomena in new and illuminating ways. He presents us with "excellent similes" (Wittgenstein) and "brilliant analogies" (MacIntyre), for example, in the case of his doctrine of infantile sexuality, an analogy between certain pleasures experienced by infants and "the satisfactions of mature sexuality" (pp. 74-75). By using the Freudian concepts to identify and classify a patient's behavior, the analyst tries to provide the patient with "a way of arranging the past which is acceptable to the present" (p. 87).

MacIntyre's criticisms of Freud's theoretical claims (he holds that the confusions in psychoanalytic theory do not seriously detract from the value of psychoanalysis as a method of treatment) seem to me cogent. His account of Freud's "positive descriptive achievement" is stated in rather general terms and is not as clear as it might have been if more concrete examples had been presented. It is, however, interesting and very suggestive.

Granted that explanations of human behavior, including psychoanalytic explanations, are generally not causal, may it not be that human actions nevertheless do have causes and that causal explanations of them can be found? MacIntyre does not discuss this question

but Peters does and gives it a negative answer. The concept of human action (behavior), he argues, is such that it is impossible in principle to give a sufficient explanation in causal terms of an action ("what a man does"), though such an explanation can be given of a "breakdown" in action ("what happens to a person"). Peters' arguments for this are interesting but not very clearly stated. We cannot, he says, "specify an action exhaustively in term of movements of the body or within the body" (p. 12). There are, in the first place, indefinitely many different ways in which a given sort of action, for instance, the signing of a contract, could be performed, and, while there may be a "general range of movements" which must occur if a contract is signed, there are "no specific movements of the muscles, limbs or nervous system, which *must* occur before it would be conceded that a contract had been signed" (p. 13). The significance of this is not altogether clear, for it would seem offhand that what Peters here says of signing a contract is also true of, for example, falling down (presumably not a kind of action), there being indefinitely many different ways in which one could fall down. But Peters gives a second argument which reveals a difference between descriptions like "He signed a contract" and descriptions like "He fell down." Action descriptions imply norms and standards and "cannot be deduced from statements about mere movements which have no such normative implications" (p. 14). I am unclear as to what it means to say that a statement like "He signed a contract" implies norms and standards, but perhaps the point is this: bodily movements of any sort whatever could occur without the signing of a contract occurring, for it is only within certain surroundings, for example, in the context of certain social and legal institutions, that anything can be called the signing of a contract, and any sort of bodily movement could occur outside these surroundings. Peters concludes that "we could never give a sufficient explanation of an action in causal terms because we could never stipulate the movements which would have to count as dependent variables" (p. 13).

It might be objected that Peters' argument proves too much, for the state of being in pain presumably cannot be "specified" exhaustively in terms of bodily movements, yet we can surely say that someone's pain is caused by, for example, a decayed tooth. This point aside, however, it seems to me that Peters' way of stating the conclusion of his argument is somewhat misleading. For it suggests that there are certain events, those that we call human actions, that are immune to causal explanation, and this Peters certainly has not shown. What his argument does seem to show is that a causal explanation cannot be

an answer to a question like "Why did he sign the contract?"— that is, to a question in which the event to be explained is described as a human action of a certain sort. But it does not follow from this that an event which can be so described is thereby immune to causal explanation. A given event (what happened at a particular place and time) can have many different descriptions. And to say that an action description is never logically equivalent to a movement description is not to say that an action description and a movement description cannot, on a particular occasion, be descriptions of the same event; "A raised his hand at t" and "A's hand went up at t" have not the same meaning, but the first does not assert that there occurred at t some event or events *other than* or *in addition to* the event described by the second. But there is nothing in Peters' argument to show that it is not possible in principle to give a causal explanation of any bodily movement, including those movements which constitute particular cases of voluntary action, and there is nothing, therefore, to rule out the possibility that in any particular case the event(s) described by an action description can be explained in causal terms.

Peters' book is in the main an attack on the notion, which he thinks has obstructed progess in psychology, that the goal of motivational psychology should be a general theory, couched in mechanical or quasi-mechanical terms, which provides a way of explaining all human behavior. He argues that there are "many *different* sorts of questions which can be asked about human behavior" and that the differences are such that "an all embracing theory is inappropriate" (p. 3). Human behavior cannot be explained causally, and the appropriate model for explaining it is the "rule following purposive model," not the mechanical model with which many psychologists (e.g., Hull, who is Peters' favorite target) have been enamoured. Peters devotes a large part of his book to an examination of the work of a number of motivation theorists, and attempts to show that these theorists have been led, by their craving for a general theory and their inattention to conceptual distinctions and the ordinary use of certain key terms ("need," "drive," "motive," and so forth), into a multitude of conceptual confusions. He has many useful things to say about reasons, motives, and so forth, and his criticisms, often interesting and acute, deserve the attention of psychologists. But he discusses, in a relatively short space, a bewildering assortment of theories and theorists (Freud, Tolman, Hull, McDougal, and McClelland, to name but a few), and his discussions are often so sketchy that it is not clear what view is being criticized or what the criticism is (fairly typical is the discussion of Guthrie, pp. 116-117).

And while Peters says a great deal about what psychologists should not try to do, his remarks about the proper task of motivational psychology are rather vague; we are given little indication of what a conceptually impeccable theory, one utilizing the "rule following purposive model," would look like.

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SYSTEMATIC THEOLOGY. Volume II, Existence and the Christ. By PAUL TILLICH. Chicago, University of Chicago Press, 1957. Pp. xi, 187. $4.50.

I wish that I could adequately review this book. Clearly, many will think it a book of the first importance. Obviously it is written by a man of deep theological understanding. But I cannot see the point of it. The work of which this is the second volume is described on the dust jacket as a "monumental reinterpretation of Christian theology." What is obscure to me is the sense in which it is a "reinterpretation." The theology in the book does not strike me as particularly novel. There is a profundity in the handling of the various topics (sin, for example, pp. 44-59) from which we can all derive valuable insights. But in general the theological position seems to belong to the "enlightened" post-liberal tradition of contemporary Protestantism. The value of the discussions of particular topics lies in their power to suggest new "perspectives," in the way in which a sermon or a play can suggest them. The element of "reinterpretation" seems to come not in the details of the theology but in the setting of the theology in a metaphysical frame; and it is this frame which I find very puzzling. Roughly, Tillich operates with the categories of Essence, Existence, and the New Being. The human predicament is described in terms of estrangement between essence and existence. Christ is the New Being in Whom this estrangement is overcome. It is into this framework that Tillich fits his (to all appearances) moderately orthodox theological picture. But what is the frame supposed to do to the picture? We could settle this question more easily if we could determine what "essential being" and "existential being" are supposed to stand for. At times "essential being" seems to mean "things as they ought to be," and "existential being" "things as they are." In this case the estrangement between the two would amount to the fact that what is is not the same as what ought to be. At other times, however, "essential being" looks more like a synonym for "God." What then is happening here? Does Tillich's reinterpreta-

tion consist in reducing God to a realm of "Platonic essences," thereby turning Christianity into a nontheistic religion? This would indeed be a reinterpretation. Or is God to be retained, His nature being somehow illuminated by the application to Him of the concept of essential being? I suspect that the truth is nearer to the latter suggestion than to the former. But my trouble is that I cannot see what illumination is derivable from the use of this "Platonic" language. This being so, I cannot see in what the reinterpretation consists. To me, therefore, the book says very little as a whole (I have already conceded the value of many of its parts), the phrases "Essential Being" and "the New Being" being no more than tiresome periphrases, on many occasions of their use, for "God" and "Christ." I am much more than half convinced that this is my fault.

In the discussion of particular theological topics Tillich's main desire appears to be to say something more or less orthodox in his own peculiar language. For this reason, when one would like to know his views on some point, one finds that it is not to his purpose to give them. For example, in his discussion of the resurrection of Christ, I gather that for Tillich the essential point is that the New Being is indestructible, and I gather also that Tillich does not believe the story of the empty tomb. This is all very well and sounds like something we have often heard before. But what does Tillich think did happen? Did the tomb stay sealed? Did someone steal the body? Did the apostles have hallucinatory resurrection appearances, or did the piety of the early church invent the accounts of these? On these crude questions Tillich has nothing to say. "Faith" is only interested in the "certainty of . . . victory over the death of existential estrangement." It does not matter what happened to the tomb. But how far does this go? Does it matter whether the Lord's body was ever in the tomb? Whether He died on the cross? Whether He ever lived? No doubt it is possible to draw the line, and I feel sure that Tillich draws it in a theologically profound place. But if he told us *where* he drew it, that would be interesting. If he told us *how* to draw the line between what may be an accretion of mythological symbols on the one hand and what must be clung to as an essential factual core on the other, that would be a truly valuable contribution to theological controversy. As it is, not to mince words, the discussion of the resurrection seems to me to be a presentation of a pretty common theological position in unenlightening language which gets us nowhere. So it is to me with the discussion of other topics, once one has discounted "insights," "perspectives," and so forth. If you ask me what is the hard conceptual content of the book, I have to answer

that I do not know. But at the same time I feel that this has been an unperceptive review.

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MODERN PHILOSOPHY OF SCIENCE. Selected Essays by HANS REICHENBACH. Translated and edited by MARIA REICHENBACH. Foreword by RUDOLF CARNAP. London, Routledge and Kegan Paul; New York, Humanities Press, 1959. Pp. ix, 214. $5.50.

This volume contains eight essays: (I) "The Present State of the Discussion on Relativity"; (II) "The Theory of Motion according to Newton, Leibniz, and Huyghens"; (III) "Causality and Probability"; (IV) "Aims and Methods of Modern Philosophy of Nature"; (V) "The Principle of Causality and the Possibility of its Empirical Confirmation"; (VI) "Rationalism and Empiricism: An Inquiry into the Roots of Philosophical Error"; (VII) "The Freedom of the Will"; (VIII) "On the Explication of Ethical Utterances." The first five are translations of papers published in German between 1921 and 1932, the sixth is Reichenbach's Presidential Address to the American Philosophical Association (Pacific Division), 1947, and the last two are papers which were unpublished at the time of Reichenbach's death. Only the sixth has been previously published in English. There is also a complete bibliography of Reichenbach's published writings.

Essay I contains critical discussions of a number of philosophical interpretations of relativity: the views of Vaihinger's followers, Mach and his followers, the neo-Kantians, and those whom the author calls "relativists" (Schlick, Einstein, and himself). Of particular interest is the sympathetic discussion of Cassirir and Kant concerning the bearing of relativity upon Kant's transcendental method.

The title of essay II gives sufficient indication of its contents.

Essay III antedates Reichenbach's famous pragmatic justification of induction; here he attempts to dispose of the problem of induction as a pseudo-problem.

Essay IV is one of the important early statements, in popular terms, of the viewpoint of logical empiricism. It deserves to be read along with such writings as Carnap's *Philosophy and Logical Syntax* (which it antedates).

Essay V is one of the finest essays ever written on the problem of causation. It supplements and is well-supplemented by Reichenbach's treatments of the subject in many of his other works.

Essay VI, published seventeen years after essay IV, is another popular account of the general philosophical orientation which Reichenbach espoused. One of the major differences between them is that the later emphasizes the central importance of Hume's criticism of induction for epistemology.

Essay VII is Reichenbach's attempt to deal with the problems of freedom of will and action. This previously unpublished paper is an important contribution to the subject, largely because it contains reasonably precise explications of causality, determinism, freedom, and so forth, and it is written against the background of a well-developed treatment of counterfactual conditionals. The main thesis is that freedom is incompatible with determinism and is compatible with indeterminism. The attempt is made to show that causality can be construed probabilistically in a manner compatible with indeterminism and that these probability relations can, under appropriate restrictions, support counterfactuals. Such counterfactuals can be used to explicate freedom; indeterminism does not destroy the important causal relation between volition and action. It is shown, however, that the attempt to construe the appropriate counterfactuals as supported by the formal implications required by determinism yields paradoxes which would exclude freedom. (An unfortunate typographical error occurs in formula 18, page 171; the consequent should be negated.)

Essay VIII is Reichenbach's final contribution to the controversy between cognitive and noncognitive ethical theories. It contains an important methodological point. Instead of taking the necessarily inconclusive line of attempting to describe what people actually do when they reason about morals or of attempting to say how ethical terms are used in ordinary language, Reichenbach tries to show that the cognitive interpretation is logically *incomplete*, that is, that the cognitive interpretation would yield a language which is inadequate to fulfill the functions of ethical discourse.

Maria Reichenbach is to be congratulated on her skill, understanding, and tireless efforts in translating and editing this book and other works of Reichenbach. (There is, in the bibliography, an impressive list of posthumous publications.) Her translations are accurate and they are made in English of excellent literary quality. Her editorial work is extremely faithful to the intentions of the author. For example, as she clearly states at its beginning, essay VIII was reconstructed from notes left by Reichenbach at the time of his death. As one who participated with Hans and Maria Reichenbach in the discussion of these ideas just

before the notes were written, the reviewer can vouch for the fidelity of the reconstruction.

To the best of the reviewer's knowledge, the final two essays in this book are the last of Reichenbach's hitherto unpublished philosophical works. This seems, therefore, a fitting occasion to express the conviction that probably no other individual philosopher has contributed as much as Hans Reichenbach to modern philosophy of science. The present volume contains valuable parts of this contribution.

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THE IDEA OF A SOCIAL SCIENCE AND ITS RELATION TO PHILOSOPHY. By PETER WINCH. London, Routledge & Kegan Paul, 1958. Pp. 143. 12s 6d.

The surprising theme of Mr. Winch's book—which is one of the *Studies in Philosophical Psychology* edited by R. F. Holland—is that the proper business of sociology is the elucidation of modes of discourse. One also learns, at least by implication, that one of the most important recent contributions, if not to sociology itself at least to the methodology of it, is Ludwig Wittgenstein's *Philosophical Investigations* (pp. 24 ff.). But this is intended quite seriously, and nowhere in the book's Chapter 3, "The Social Studies as Science," which is the heart of the book—that part of it in which the author's views are given their most systematic exposition—is there the slightest indication that sociologists have some interest in the development of institutions. The only alternative to his view which is taken seriously enough to be discussed in this chapter is a sort of reductionist mechanism (pp. 75 ff.). That social scientists may not be expected to predict the direction that jazz will take or the writing of a specific poem is taken to be reason enough to rule out nomothetic social science (p. 93 f.). But while these might well be goals of a social science informed by principles such as those of Holbach or LaPlace, I must confess never to have seen this kind of thing given as the goals of social science as actually practiced today.

Winch argues that social science must comprehend the social situation it seeks to deal with in the way in which it is understood by the actors themselves (ch. 2). To be sure, this is not to be understood as meaning that social science merely records unthinkingly human acts which are performed unthinkingly. Implicit in every social act are rules which determine that this is the sort of thing that one does in the kind of situation confronting the actor. Usually, the actor acts without

troubling to take account of the rules. But should he be challenged to defend his course, he might well be able to, and he would offer such defense as he could in terms of rules. The more sensitive he is to the rules which govern ordinary behavior in his society, the better the account will be that he offers. The sociologist is presumably one who takes the greatest pains with this sort of thing; he is one who is trained to elucidate the rules of the social game. This is on the analogy of the elucidation by linguistic philosophers of the rules of the various language games we are said to play. Sociology properly done, then, is just a special application of linguistic philosophy. (This view of sociology, it may be noted, is not to be mistaken for the perfectly sound view that some questions are to be answered by conceptual analysis and not empirical research—pp. 17 f.—and one is at liberty to accept the latter without the former.)

How can the new conception of sociology that is offered in this book be justified? I think it must be admitted from the start that any argument which has as its conclusion "such and such is the nature of social science" and yet does not purport to be a description of what social scientists are doing must perforce be a *petitio principii*. This is the case for Winch's argument. He will argue against those who believe in the possibility of a nomothetic social science that social science can only be concerned with the elucidation of rules (not, of course, laws) governing social behavior. The conclusion is assured by the simple expedient of making sociology a discipline which does not seek to *explain* social phenomena but rather to *understand* the actions of human agents. As we have seen, it is concerned to elucidate the argument that a rational actor might give in defense of his course of action were he fully aware of the rules of social conduct in his society. Thus it is concluded that sociology is only what Winch will permit sociologists to do.

The fact of the matter is, however, that sociologists seem not to be aware of this restriction upon their activities and seek—with whatever success—to do other things. And some take it to be entirely within their purview to seek explanations of social phenomena. That is to say, they are concerned to discover, among other things, how it was that this or that institution came to be as it is or how institutions of determinate type develop and change. But the kinds of problems that these inquiries are called upon to solve cannot be solved through the elucidation of rules in Winch's sense.

The existence of a social rule presupposes the existence of an ongoing sociocultural system. (I use "existence" here only for ease of expression

and not in order to give anyone metaphysical jitters.) It is possible, I suppose, to consider such a system as a system of rules (cf. p. 127 f.). They suppose that human actors more or less know what sort of behavior to expect from normal fellow-men with whom they share their culture. But if I want to account for the existence of an institution, or if I want to explain the existence in this society of some particular rule, I cannot appeal to the rule to explain itself. Nor can I look for another rule of the same sort to explain my first rule. (At best, the rule can make intelligible the actions of an agent whose actions presuppose rules or institutions, though I suspect that the way Winch conceives of this it can only be done on the assumption that the agent is rational in all relevant respects.) To explain the presence of institutions, I must be able to specify the conditions which give them rise. Thus, even if we allow Winch's claim that what the sociologist studies is determined (not in the causal sense) by rules other than those of his investigation (p. 87), it is clear that if he is not permitted to go beyond the given rules he cannot explain them.

We have seen that Winch's argument is a *petitio*, but why should he wish to formulate an argument such as the one he does? That is, why does he wish to exclude nomothetic social science? The kinds of examples he uses in the course of his discussion suggest as one possible answer that he does not really appreciate that there can be diachronic studies of institutions. The problems he recognizes seem to be only those of individual actors, even though he takes social rules to be non-individualistic in character (p. 128), and it is clear that he makes no serious effort to distinguish between sociology and psychology. It seems suggested that a social science which was not merely the elucidation of rules would be a science which was concerned to uncover those causal factors which compel men to act as they do, and that this "idea of a social science" would be not only repugnant to our moral consciousness but would be at variance with our sense of not being compelled. It is worth repeating that the example that Winch offers of what is to be rejected in favor of his conception of social science is rather primitive mechanistic psychology (pp. 75 ff.) which, however bad psychology it may be, is not sociology at all. Winch's point seems to be that men are not pushed about, either by inner forces or by the sort of external powers that J. W. N. Watkins has characterized as "holistic" (*British Journal for the Philosophy of Science*, VIII, 1957, 106 f.). Rather he believes that they can, and often do, offer a fairly good account of themselves. But he fails to recognize that the account is always within a system of institutions which are taken for granted. Sociology is that

discipline which, among other things, is supposed to account for such institutions. And it certainly cannot be claimed on the basis of anything that Winch has said that a nomothetic social science not having the repugnant features mentioned above is impossible.

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ESSAYS IN MORAL PHILOSOPHY. Edited by A. I. MELDEN. Seattle, University of Washington Press, 1958. Pp. xii, 216. $4.50.

Professor Melden is to be congratulated on the successful outcome of a difficult and unpromising venture. Arguing that too frequently able men waste their energies on the all too familiar question of how inquiry should proceed, he invited some "whose competence had been established" to address themselves to several "substantive and perennial problems of moral philosophy." Three philosophers from the United States, three from Britain, and two from New Zealand, all of them well known, some famous, responded to his invitation, producing eight self-contained essays on mostly unrelated subjects. Yet these eight solid courses, prepared by eight different cooks employing very different national and personal styles, experimenting with a variety of methods and techniques, catering to very different tastes, working without mutual consultation and without the unifying direction of a chef, nevertheless blend into a rich and satisfying if perhaps not an evenly exciting meal.

Gilbert Ryle always deals with important problems and always in a fresh and lively manner. This article ("On Forgetting the Difference between Right and Wrong") is no exception. He produces two combinations of ideas: (a) forgetting the difference between right and wrong, and (b) morally miseducating someone in a major moral matter, both of which he regards as absurd. If they are, this throws light on the nature of acquiring knowledge in moral matters. I myself, however, can see nothing absurd in the idea of moral miseducation. I believe that, for instance, the head-hunters of Borneo, the Romans, and the Nazis have miseducated their children in important moral matters, such as taking human life, slavery, and the treatment of racial minorities. In any case, what is the cash value of Ryle's claim? That people cannot in fact be made to believe that slavery or head-hunting is right? Or that they have no excuse when they come to believe it? Or that there is no such question as whether or not such a belief is true or false?

Concerning the main absurdity Ryle claims that forgetting the difference between right and wrong is not comparable to forgetting a memorable fact such as one's own name or a simple skill like one's own language. In his view *there is no such thing as* forgetting that difference (p. 149). Of a person who knows the difference we expect that he will do right and shun wrong. When he ceases to do so, then, says Ryle, we must never explain this as a case of forgetting the difference. Knowing the difference between right and wrong is analogous, in his view, to knowing the difference between good and bad poetry. Teaching that difference involves such things as "inspiring, kindling, and infecting" (p. 154), and knowing it such things as "appreciating" and "caring" (p. 156). Of people who know it we expect that they should read good and shun bad poetry. When they cease to do so, it would be absurd to say that they had forgotten the difference (p. 151). "Ceasing to care is not forgetting, any more than ceasing to believe something or to mistrust someone is forgetting" (p. 156). But all this proves only that a person may behave as if he had forgotten the difference between good and bad poetry although he has not forgotten but has simply ceased to care about it. True, a man who ceases to believe something has not forgotten it, but this does not show that *there is no such thing as forgetting*.

A much longer, more detailed, and more thorough treatment of the same and related problems is given by William K. Frankena ("Obligation and Motivation in Recent Moral Philosophy"). The question is the one at issue between "Externalists" and "Internalists": "What the externalist must deny, and the internalist assert, is that having objectively a certain moral obligation logically entails having some motivation for fulfilling it, that justifying a judgment of objective moral obligation logically implies establishing or producing a motivational buttress, and that it is logically impossible that there should be a state of apprehending a moral obligation of one's own which is not accompanied by such a buttress" (pp. 73-74). This issue goes very deep indeed. It is also alluded to by H. L. A. Hart ("Legal and Moral Obligation") when he says "To characterize morality (as, for example, R. M. Hare does in his illuminating book, *The Language of Morals*) as *primarily* a matter of the application to conduct of those ultimate principles which the individual accepts or to which he commits himself for the conduct of his life seems to me an excessively Protestant approach. Important as this aspect or kind of moral judgment is, we need to understand it as a development from the primary phenomenon of the morality of a social group" (p. 100). The very nature and

point of morality itself is here at issue. Frankena's statement of the comparative disadvantages of the opposing viewpoints seems to me clear and fair: "Externalism . . ., in seeking to keep the obligation to act in certain ways independent of the vagaries of individual motivation, runs the risk that motivation may not always be present, let alone adequate, but internalism, in insisting on building in motivation, runs the corresponding risk of having to trim obligation to the size of individual motives" (p. 80). Frankena also seems to me right when he suggests that controversies of this kind require that we achieve "clarity and decision" about the larger social and psychological contexts in which the phenomena of individual and social morality are embedded (p. 80).

Marcus G. Singer ("Moral Rules and Principles"), like Ryle, is interested in moral epistemology. He hopes, by an examination of the role played by moral principles in the justification of moral rules and individual acts, to show how we can come to know whether given acts or rules are morally right or wrong. He has unearthed particularly illuminating examples (pp. 183-187) to show how one principle, which he calls the generalization argument, can be used to justify very different rules for the use of commodities in conditions of abundance and of scarcity.

I doubt, however, whether his main distinction between moral principles and moral rules is tenable. He makes it a defining characteristic of moral principles that they hold in all circumstances and allow of no exceptions, are invariant with respect to every moral judgment and moral situation, are therefore "indefeasible," and incapable of conflicting with one another (pp. 169, 192), whereas moral rules are merely "propositions to the effect that a certain kind of action is generally right or wrong" (p. 164), which is "equivalent to saying that any action of that kind is (right or) wrong unless there is a reason to the contrary" (p. 165). "Thus moral rules do not hold in all circumstances, are not invariant, are defeasible" (p. 169). Singer's own statements of various moral principles make them indistinguishable from moral rules. The generalization principle runs thus: "If not everyone has the right to act in a certain way, then no one has the right to act in that way *without a reason or justification.*" But this is precisely the formulation for a moral rule: a proposition to the effect that an action of a certain kind (doing what not everyone has a right to do) is *generally* wrong, that is, is wrong unless there is a reason to the contrary. What is the difference between the *principle*, "If not everyone has the right to act in a certain way, then it is wrong to act in that way unless there is a

reason to the contrary or a justification" and the *rule*, "If not everyone has the right to act in a certain way, then it is *generally wrong* to act in that way"?

Hart, in the important essay I have already mentioned, hammers home the timely point that a moral obligation is not any and every kind of moral tie but only a highly specialized one, and duty is another (pp. 103, 104). He throws into relief the three salient features of a legal obligation and shows that typical moral obligations, promises, have analogous features (pp. 100-103). One of these features, however, is importantly different; it has a very special sanction. Moral coercion or pressure appeals not to fear of harmful consequences, "but to the delinquent's presumed respect for the rules he has broken" (p. 103). "As soon as an appeal is made primarily to fear even of unorganized 'sanctions,' we are on the way from moral to legal obligation" (p. 103). This seems to me an important and illuminating point. I want merely to add that a moral obligation is not automatically changed into a legal one when the legal sanction is added, as the words "on the way from moral to legal" perhaps suggest. Our moral obligation to keep a promise is not superseded by the legal one when, as in cases technically known as "breaches of promise," the promisee is protected by a legal sanction, any more than killing ceases to be morally wrong when it is made punishable by law.

J. O. Urmson ("Saints and Heroes") draws attention to and neatly defines two kinds of conduct, saintly and heroic, for which "simple utilitarianism, Kantianism, and intuitionism have no obvious theoretical niche" (p. 207). They can speak about acts that are obligatory, forbidden, and permissible, but not those that are "supererogatory," yet this fourth category is indispensable. What we consider basic moral duties are those requirements whose fulfillment constitutes "the only tolerable basis of social life" (p. 215); hence we may legitimately demand them of one another. Beyond them lies the ideal: "A line must be drawn between what we can expect and demand from others and what we can merely hope for and receive with gratitude when we get it; duty falls on one side of this line, and other acts with moral value on the other, and rightly so" (p. 213).

Richard B. Brandt ("Blameworthiness and Obligation") and G. E. Hughes ("Moral Condemnation") in long, closely-argued, and thought-provoking papers, cover roughly the same ground, the various things we can say and do about or to people who have done something wrong. Brandt aims to arrive at a definition of "*x* is morally blameworthy on account of *z*" and Hughes to lay bare what it is morally to

condemn something. Interesting and important though these investigations are, they suffer from a serious defect. Neither Brandt nor Hughes, in stating the definitions or the logical implications of their key terms, blameworthiness and condemnation, make clear which of the many possible uses of these words they are trying to clarify. Lack of space unfortunately prevents me from even summarizing their valuable papers.

A. N. Prior ("Escapism: The Logical Basis of Ethics") serves an elegant logical trifle to round off this meal for those with a taste for symbols. Unlike the other essays, his is not about some perennial problem of ethics. He wishes to construct a logical calculus suitable for the propositions and arguments of morality. His main contribution is the examination and development of a "spectacular simplification," invented by A. R. Anderson, which consists in substituting for the usually undefined concept obligatoriness the concept of being sanctioned. Thus, "p is obligatory" becomes "omission of p necessitates the sanction," which is equivalent to "avoiding the sanction necessitates performance of p" and this to "escaping necessitates p." This makes possible many further simplifications but it also gives rise to problems. It does, for instance, land us with the delightful "Paradox of the Good Samaritan": anyone helping a person who has been wronged is doing wrong. Prior's solution is that "each person should regard deontic logic as applying to those measures *he* must take to avoid the sanction that threatens *him*" (p. 144). Since the sanction is unspecified and might be anything from having one's head chopped off to a mere "failure to be perfect" (p. 146), we can see that "the logic, the pure structure of 'being perfect' is the same as the pure structure of escaping from what we fear, so that the same calculus can be turned indifferently to the one use or the other. This is worth coming to know—for there is a beauty in such parallelisms which it is a pleasure to contemplate—even if it leaves us none the wiser as to precisely what we ought to do" (p. 146). In this sense, then, and this only, is escapism the logical basis of ethics. It would perhaps have been less misleading to speak of "the basis of deontic logic." Moreover, one might have spoken with equal justification of perfectionism as the basis of deontic logic, or of escapism as the basis of perfectionism, or a good many other things. Why bring in ethics?

The book unfortunately has neither an analytic table of contents nor an index.

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MEDIEVAL THOUGHT FROM SAINT AUGUSTINE TO OCKHAM. By GORDON LEFF. Baltimore, Penguin Books, Inc., 1958. Pp. 317. $.85 (paper).

It is impossible to treat the medieval period of Western intellectual history with any completeness and adequacy in 317 pages, and it is not to be expected that this little book do more than the modest task its author has set for himself in his Preface. And on the whole, a fair picture of the development of medieval philosophy emerges from his pages. It is a pity, therefore, that I am obliged to call attention to a number of errors of fact and interpretation that will be encountered in the book.

A few misleading remarks occur in Chapter 2 in Mr. Leff's account of Augustine. On page 43, he calls the doctrine of seminal reasons Platonic whereas it is really a Stoic and Neoplatonic conception. He also describes the Augustinian doctrine of cognition in terms of *reminiscence*. While not absolutely mistaken, this is surely misleading since the doctrine of the interior master was Augustine's alternative to the Platonic doctrine of reminiscence which, as a Christian, Augustine could not accept.

In a book purporting to give a complete survey of the main developments, I regard the omission of the Apostolic Fathers and the Cappadocian Fathers as serious. In the case of the former, it is important for the student of medieval thought to see that the struggle between Christian humanists and Christian anti-humanists dates from the earliest Christian times.

In Chapter 3 the emanationist character of the writings of pseudo-Dionysius is not made clear. It is not made plain that Eriugena's doctrine of the "return" (the Neoplatonic *epistrophe*) is an automatic and necessitated affair. I am aware that there is some debate possible here, but the point is crucial and should at least have been mentioned.

I do not feel that the complexity of Damian's views has been sufficiently appreciated. If Leff had concentrated attention on the problem of the power of God, Damian's doctrines would have been more satisfactorily explained. The account of Anselm's ontological argument on page 101 is deficient, and I doubt very much whether an uninitiated reader would have much of an idea of it from Leff's account. The account of Abelard (pp. 109-111) is defective in several particulars. Abelard did not exactly hold that the *status* deals with general characteristics. Furthermore, and despite what the author states, Abelard's solution of the problem of universals *does* go beyond purely logical

considerations because he explicitly stated that individuals themselves are exactly resembling (*ipsi invicem conveniunt*). Vignaux, who has given the best account of the doctrine, correctly compares Abelard's formula with Ockham's. Again, despite what Leff asserts, Abelard *did* have some important things to say about the sources of human knowledge (see, for example, the first 30 pages of *Logica ingredientibus*). Leff is correct in not calling Abelard a moderate realist, but he should have said a little more about the doctrine of abstraction. And it would have been worthwhile, especially in a general survey, to have pointed out Abelard's humanistic side as it comes out in the letters to Heloise. Abelard's essential dependence on Boethius and Gilbert's on Porphyry should have been indicated.

The account of the contribution of Islam is, inevitably, somewhat sketchy, but here, too, some mistakes must be pointed out. Al-Ashari and his school are said to have "closed the door to all speculation in theology" (p. 143). As a matter of fact, however, some of the most original thought in Islamic philosophy derives from the Kalam, especially from the Asharites. The critique of causation in Al-Gazali's *Tahafut* comes from the Asharites, and anyone who has read it must have noticed its close resemblance to similar investigations by the fourteenth-century scholastics, Malebranche, and Hume. No mention is made of Al-Kindi's treatises on magic, which are extremely important for later developments; they throw considerable light, for example, on Leibniz's monadism. The explanation of Avicenna's "Equinitas est equinitas tantum" as meaning "A horse is simply a horse" is both incorrect and quite misleading.

As regards Al-Gazali, Leff is in error about the relationship between the *Tahafut* and the *Mahasid*. He states, "His [Al-Gazali's] *Destruction of the Philosophers* was intended only as a preliminary to his attack on the philosophy associated with Avicenna. It consisted very largely in an exposition of Avicenna's views; and unfortunately for him only this part—the *Intentions of the Philosophers*—reached the West . . ." (p. 133). What Leff should have said is this: the *Intentions* was a preliminary exposition of Avicenna. It was this that was translated into Latin. The *Destruction* was the attack on the philosophers (not a preparation for such an attack) and *it* was not translated into Latin. Moreover, the preface to the *Intentions* was omitted in the Latin and this omission was responsible for the erroneous impression that Al-Gazali was an Avicennian adherent.

Concerning Averroes, Leff again conveys the wrong impression (p. 156). Averroes explicitly rejected the "double-truth" doctrine in

his *Decisive Tract*. But, as Simon van den Bergh has shown, Averroes' position is sufficiently ambiguous to give rise to such a doctrine. Doubtless some of the misunderstanding in the Latin West was due to mistranslations of some references to "the theologians," but even so, Averroes himself sometimes appears to want to be all things to all men. But contrary to what is said on pages 156-157, Averroes did *not* deny the harmony of faith and reason.

Chapter 7 on the characteristics of the thirteenth century is generally sound. But here, too, we find some misleading and incorrect statements. Grosseteste is alleged to have held that the soul could not derive knowledge through the senses (p. 189). As this stands, it is mistaken. Anyone who reads Grosseteste's commentary on the *Posterior Analytics* is aware of the indispensable importance of sense-experience in Grosseteste's theory of scientific knowledge. Again, I do not understand the meaning of the statement that "Hylomorphism was essentially a reaction against the Arabian Separate Intelligence" (p. 193). The doctrine that matter is universally encountered throughout the hierarchy of being from the Intelligences on down the scale was already in Plotinus and is encountered throughout in Islamic philosophers of the Neoplatonic persuasion.

The estimate of the Franciscans of the thirteenth century is judicious and puts appropriate emphasis on Bonaventure's exemplarism, his insistence on the primacy of Being in the order of knowing, his view of illumination (as sapiential wisdom in contrast to Aristotle on knowledge), on light as the primary form of body, on plurality of forms, and the essentially active nature of the soul. Leff correctly describes Aquinas' main contribution: Aquinas did not regard the sensible world as relatively shadowy but rather as "the surest evidence open to us of reality" (p. 214). But his statement on page 217 that Aquinas conceives being as hylomorphic, although corrected on page 222, is still misleading (a) because it neglects the doctrine of the twofold composition (viz., that of potency and act and that of matter and form), and (b) because it neglects the denial of hylomorphism in separate substances.

Leff is, of course, correct in noticing that the so-called "Latin Averroism" is a dubious characterization of the views of Siger of Brabant and Boethius of Dacia. But there is surely something to the view that there were adherents to the strictly naturalistic interpretation of Aristotle, and here at least some conjecture would not have been out of place.

The later period (fourteenth century) is characterized as skeptical and, if carefully qualified, this can be regarded as correct. But the

statement that "The fourteenth century carries no direct impress of the thirteenth" is plainly wrong. As Duhem, Gilson, Baudry, Vignaux, and others have shown, the character of fourteenth-century thought is decisively determined by the condemnations of 1270 and 1277 (the "articles of Paris").

Scotus' proof for divine existence and his *formal distinction* are not explained with any clarity. The nontechnical reader will therefore get little from these discussions and nothing that is really accurate. Leff is wrong in ascribing to Ockham the view that God is in no sense a subject for reason (pp. 258-259). As a matter of fact, Ockham offers what he regarded as a demonstration of the existence of God in his "Questions on the Sentences" and again the same argument in his "Questions on the Physics" (a much later work).

On pages 269-270, Leff has confused the doctrine of plurality of forms with the doctrine of the formal distinction. Now the facts are: (1) that there is only a *formal* distinction among the powers of the soul (rational, sensitive, and vegetative) and (2) that there is a *real* distinction between the form of corporeity and the soul in the composite substance which is man (*Opus oxeniense*, 2, 16, no. 17). The statement on page 281 that Ockham rejected metaphysics is false: (1) Ockham stated explicitly that he intended to write a book on metaphysics; (2) in any normal acceptance of the term, there is an explicit metaphysics in Ockham's extant writings. It is a mistake again on page 287 to say that Ockham denied the formal distinction in God's nature.

I am very sorry to have to notice all these errors in the review, but it is the duty of a reviewer to scrutinize popular as well as technical books in this way. It is possible to give a popular account of medieval thought that is not mistaken in matters of detail. P. Vignaux has done it. A little care could have avoided or eliminated most of these mistakes.

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THE IDEA OF FREEDOM. By MORTIMER J. ADLER. New York, Doubleday and Co., 1958. Pp. xxvii, 689. $7.50.

This is the first published work from Professor Adler's Institute for Philosophical Research, and its comprehensiveness provides a fair basis for evaluating the work he and his colleagues have been doing since 1952.

Nearly a hundred pages are devoted to explaining the general procedures used at the Institute (round table discussions by proponents

of diverse views), and the "dialectical" method employed in producing the book (discovering common characteristics among authors). He claims only to be presenting "an accurate construction of philosophical controversy," although he hopes this will establish a "methodological reform" which will make philosophy less like poetry and more like science, since differences of opinion will have been clarified and hence can be adjudicated.

The bulk of the book explicates the ideas, and classifies the writers, on freedom. Three principal ideas are recognized: (1) Circumstantial Freedom, where "an individual's freedom depends upon whatever external conditions affect human behavior"—and there are further sub-divisions into those writers who do or do not (1) assert self-realization, (b) mention psychological or social limitations, (c) distinguish degrees of freedom, (d) consider the effects of law, and so forth; (2) Acquired Freedom, a freedom which is possessed "only in conjunction with a certain state of mind that marks one man as somehow 'better' than another"—these "self-perfection" authors vary, however, according to whether or not (a) knowledge is virtue, (b) the Self is autonomous, (c) perfection takes place within a society, (d) motives need perfecting, and so forth; and (3) Natural Freedom, a freedom "that all men possess insofar as they always actually have the inherent ability in which it consists"—this is the freedom of "Self-determination," although it takes different forms depending on the author's attitude toward causality, rationality, creativity, and the like. Two political freedoms, liberty and collectivity, are variants of (1) and (2), respectively.

The final portion of text discerns the generic meaning common to all writers: "A man is free who has in himself the ability or power whereby he can make what he does his own action and what he achieves his own property" (p. 614). The book is completed by 70 pages of Bibliography and Index.

The book can be very valuable, depending on the purposes of the reader. Since the relevant writers have been collected, collated, and classified, information about a particular author's concept(s) is readily available (and sometimes surprising), while similarities and differences among writers can be determined with ease and specificity. In addition, the distinctions are useful for understanding the complexities of the subject. All in all, the book is a carefully documented and analyzed statement of the alternative meanings of freedom.

It is difficult however, to see how the book can ever be more than that. After straining at 600 pages of ponderous prose, one is not likely

to swallow the generic definition without a rueful smile, and a lament. And after one has discovered into which cubbyhole a given author is put (Bentham, for example, into Circumstantial Self-Realization, Rousseau into Natural Self-Determination and Acquired Self-Perfection, Locke into four categories, along with Aquinas) what more is there to say? Classifications serve a useful function in both philosophy and science, but to think that these provide a sufficient, or even a necessary, prerequisite for knowledge is to ignore the role of creativity in both disciplines. And to suggest that methodological reform can take place when we know that different definitions are different hardly seems justified.

Most importantly, the book, for all its impeccable scholarship and careful analyses, adds little, beyond detail, to what most philosophers already know. Even a cursory reading of the literature demonstrates that some writers, like Hobbes and Mill, believe one is free when he can do what he wants; that others, like Aquinas and Spinoza, believe he is free when he does what he should; and that nearly everyone who has written on freedom has been concerned with the problem of free will. Yet it has taken one Director, one Associate Director, two Assistants to the Director, eighteen Research Fellows, one Librarian, two Secretary-Treasurers, four Secretarial Assistants and ??? dollars to distinguish these three ideas!

Perhaps future publications will justify this group's efforts; the present work does not. Nor does the next volume look any more promising for, apparently, it will be devoted to demonstrating that there are "genuine disagreements" among the writers on freedom.

CHARLES H. MONSON, JR.

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